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Earth Surface Dynamics Discussions

Interactive comment on "Quantifying Restoration Success of Wood Introductions to Increase Coho Salmon Winter Habitat" *by* Russell T. Bair et al.

Anonymous Referee #2

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General comments

The manuscript details a study built around stream restoration efforts, and aims to evaluate the effects that large wood placement has on hydraulic habitat for fish. To this end, the authors apply a 2-dimensional hydraulic model, calibrated based on field observations. To assess the relevance of altered channel hydraulics for fish habitat, modeled flow characteristics are linked with empirical information on fish swimming performance and bed material size (to assess its mobility).

I agree with the authors that better understanding of the hydraulic effects that large wood has on stream processes is an important subject. From the basic science point of view, this topic is of interest because large wood is a key driver of many physical and biological processes in river ecosystems. Likewise, this topic is also critical from the

applied river science perspective, because large wood placement to enhance fish habitat is, by far, the most common channel restoration activity (at least in the geographical regions I am most familiar with).

The methodology applied in this study seems to be generally robust, although some additional information on model limitations and uncertainties would be desirable, to provide readers with more complete information. Similarly, interpretations and conclusions appear to be supported by the data, but I would encourage the authors to elaborate further on this in the context of model limitations. From the technical point of view, the manuscript is written well and has good quality figures that convey key results effectively. However, the manuscript would benefit from exploring in more depth some "pockets" of relevant literature to better contextualize the results. Below, I expand on all the above concerns in more length and give some suggestions.

Recommendations:

1. Methodology and interpretations. Numerical modeling of flow around large wood is a highly challenging task and there have been relatively few attempts to resolve such flow field in 3D. Thus, in my view, the 2D approach adopted by the authors can be still considered current research standard (e.g., Hafs et al., 2014; Wall et al., 2016). However, as the authors acknowledge, there are clear issues related to modeling highly complex, 3D flow using depth-averaged model and substantial errors can be expected as some assumptions are violated, at least locally (e.g., Shen and Diplas, 2008). Given the importance of this issue, I think the authors devote too little discussion to this limitation.

I would recommend that the authors discuss how the modeled flow field resembles or deviates from the patterns observed in various field studies (Daniels and Rhoads, 2003; 2004a; 2004b; Manners et al., 2007) or in experimental setting (see references below). What are the key uncertainties in the predicted flow given what we know about 3D flow structure around such obstructions? What are the implications for the predicted

hydraulic habitat? After all, fish utilize 3D habitat and can adjust their vertical position in the water column. While these uncertainties certainly do not constitute a disqualifying problem, in my opinion, they need to be signaled to readers more clearly and in more detail, so that they can more readily formulate their own judgement regarding the results.

It would be also informative to know how much of the changes in flow hydraulics (and habitat) occur in close proximity to large wood, where errors are likely large, and how much in the far field, away from the wood? For example, is there a way to plot errors in velocity (modeled-observed) against distance from wood, to get a sense of the spatial extent of the zone where flow properties are not captured well? For example, (Xu and Liu, 2017) showed that flow field predictions away from large wood may be reasonable even if a simple solid body representation is chosen.

2. Literature. The authors generally did a good job presenting most relevant literature but I feel that it is slightly less comprehensive on the numerical modeling side. Because modeling is at the core of this paper, I think the paper would benefit from exploring this literature both for providing background to the reader and for contextualizing the results. Allen and Smith (2012) and (Xu and Liu 2016; 2017) are examples of good recent references to cutting-edge approaches to tackle the challenge of modeling flow near complex features like large wood.

In addition, hydraulics of large wood, and particularly engineered log jams (which tend to have simple geometry) bear some similarities to flow around abutments and spur dikes. These parallels have been widely recognized and utilized in the geomorphic literature, e.g. see Abbe and Montgomery (1996) or Buffington et al. (2002). This kind of flow obstructions has been extensively modeled using CFD and engineering literature can serve as a rich source to draw upon in research on large wood; such modeling efforts have also been carried out by river scientists studying restoration structures such as deflectors – see work of Biron and colleagues (Biron et al. 2009; 2012).

3. Uncertainties. The authors should be commended for evaluating model performance on a number of occasions. I think it may be useful to provide more information about this important step of CFD model application. For example, I suggest that the authors consider providing information on the number of measurements used for evaluation (e.g., number of velocity measurements) and the slopes of the regression lines. The latter might be relevant, because bias in modeled velocity, relative to observed velocity, can lead to over- or underestimated heterogeneity in the modeled flow field. For example, if the slope in a modeled conditions prior to wood placement is 0.6 and, after large wood placement is 0.8, this needs to be taken into account when comparing the differences in complexity of flow field due to large wood placement.

I even wonder if it may make sense to carry out a separate comparison based on field data alone and then another one based on the modeled data, and see if those two results converge; of course, this is only if there are enough data to run reasonable regressions based on field data alone. I also noted that the reported velocity errors seem much higher than those for WSE, which should also be highlighted in the discussion, since velocity affects both aspects of habitat that are of interest here (bed shear stress is a function of velocity squared). The errors are within the range reported in the literature, so the magnitude of errors itself is not alarming, but this issue should be communicated clearly in the text. Also, personally I find that showing the data graphically is often as informative as reporting statistics (or more). I leave it to the authors to decide on the most appropriate course of action.

4. Flow event choice. Lastly, I would recommend that the authors further clarify the ecological relevance of bankfull flow for answering their research question.Why was it chosen for this paper out of a wide range of discharges a rainfall-dominated stream may experience during the winter season? Of course, this does seem like an intuitive choice for bed mobility modeling. However, it is slightly less clear why that would be the key flow for fish. Bankfull flow in wet coastal streams in Oregon has, on average, recurrence interval of $\sim$1.2 years (Castro and Jackson 2001) and in pluvial hydrological

regime flows in excess of that discharge probably last a few days per year. If bankfull flow is critical because of limited flow refugia, or was chosen because of its relevance for sediment transport and because changes in habitat patterns at lower discharges are similar, that should be clearly conveyed in the manuscript. I think it is important for readers to be able to understand broader importance of the reported results, how they extend beyond just a single flow event.

Minor comments & suggestions:

p. 1, line 25-27: LW also influences bed texture – consider citing work of (Buffington and Montgomery 1999).

p. 2, line 5-7: interesting work on LW removal effects by R.D. Smith and colleagues (Smith et al. 1993a, 1993b)

p. 2, line 8-12: I think the clarity of this paragraph would be improved if the authors added a sentence that stated clearly that low velocity habitat is critical for overwinter juvenile survival. This is perhaps a minor point but for the readership of ESD not familiar with fish ecology can be helpful in following the logical flow of this argument (overwinter survival of juveniles key for population viability & low velocity important for juvenile survival => low velocity habitat critical for population recovery).

p. 2, line 16: work of Sommer et al. and Jeffres et al., while undoubtedly interesting and relevant, should be cited with caution in this context, since it was conducted on a very different river system in different climate (floodplains of larger rivers in California Central Valley).

p. 2, line 31-34: I want to point out excellent work by A. Finstad and colleagues on the importance of bed shelters for salmonids, although, of course, there may be some differences betwee Atlantic salmon and Coho (Finstad et al. 2007; 2009)

p. 3, line 12: suggest reporting in units of mm or m, not cm.

p. 3, line 14: how, specifically, are the study reaches geomorphically distinct? Please

clarify

Table 1: what is bankfull area?

p. 6, line 20: the equation (3) defines Cf parameter, then text (e.g., line 27 on that page) refers to Cd – are those the same? Or is this just a typo? Please fix or clarify.

p. 7, line 1-3: given that large wood is at the heart of this study, it actually would be interesting to evaluate also flow field around wood. Once again, poor performance in those areas is to be expected and, in my view, does not disqualify this or any other similar work using 2D, given very limited alternatives, but it would be informative to know the magnitude of errors an spatial extent of the zone within which flow parameters are not modeled reliably. For example, one could evaluate model prediction near and away from LW, or compare evaluations including and excluding near-LW data points.

p.7, line 7: depth threshold of 0.1m seems somewhat high for juvenile Coho (they can certainly swim in shallower flows). But perhaps there is also another reason/criterion why this cutoff was chosen?

p. 9, line 12: perhaps “robustness” not “resiliency”?

p. 11, line 9: could the authors clarify whether/how gamma distribution was fitted in cases of bimodal data?

p. 12, line 4 (and elsewhere): I would encourage the authors to refer to “modeled” or “simulated” habitat rather than habitat. This may seem like hairsplitting but I think it would be prudent to emphasize that these are model predictions rather than empirical data.

p. 15, line 8: “processed” not “process”

Throughout the paper, the authors use v and u for downstream and cross-stream components of the velocity vector (e.g. equation (1) but later v also comes up to describe swimming velocity criteria for fish. The authors should be careful here to avoid using

the same symbol for different variables – please fix or clarify.

In sum, I want to emphasize once again that I believe that, upon revisions, this manuscript could be a valuable contribution to the literature. It focuses on important subject within the field of ecgeomorphology and the methodological approach it adopts, despite some limitations, is scientifically defensible and in line with current research practice. As a result, I believe the reported results are robust and will be of interest to the readership, especially researchers interested in topics at the intersection of earth surface processes and ecology. I look forward to seeing authors' responses as well as the revised manuscript.

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