A point-by-point response to the reviews including a list of all relevant changes made in the manuscript

Comment from Anonymous Referee #1

This study attempts to model a very challenging problem of wood accumulation and flood hazard in extreme events , using a devastating 2017 event in Japan as the case study. The model builds on some previous work to define the boundary conditions and describe the quantities and size distributions of the sediment and wood accumulations that occurred during this event. As such I think it is an important attempt to offer a practical model for dealing with highly uncertain but incredibly damaging events that exceed the design conditions for which bridges and channels were designed. Ultimately the effort falls short in a few different areas in my opinion and the assumptions should be better justified and tested as part of this effort.

Answer to Anonymous Referee #1

We really appreciate the time and effort you have dedicated to providing insightful feedback on ways to strengthen our paper. In this revision, we tried to explain all the unclear points in this paper that you have pointed out. Below is a point-by-point response.

11 – 'aims to propose' could be just 'proposes'

Answer; We revised the manuscript.

21 – not clear to me in the abstract what type of model you are proposing. Is it a wood budget model or is it a flood level (hydraulic) model, or both?

Answer; We revised the manuscript to make the point clearer. The following sentence was added; 'This study proposes a new method to simulate the behavior of numerous large wood pieces in the flow field with active sediment transportation by employing the convection equation and the storage equation for large wood.'

27 – should use past tense to describe 2017 event.

Answer; We modified all the relevant descriptions in Section 1.

48 – I would clarify that these are modelling Lagrangian motion rather than tracking. There has been other research attempting to physically track the motion of wood, but that is not what is being reviewed, so you should be clear that this is limited to numerical models. Note as well that there are other wood budgeting efforts that don't use Lagrangian approaches, but

do look at trapping efficiencies and the like. I'm thinking of the Benda et al papers on stochastic forcing of wood budgets in watersheds. It would be good to see this new contribution placed in that sort of context in the literature review.

Answer; Thank you very much for the suggestion. We added Benda et al papers as a reference, and tried to clarify the author's points in the manuscript.

52 – do you have an estimate for how many? At what point is the Lagrangian method infeasible?

Answer; We added the estimation regarding the large wood supply in this event in Section 1. In addition, we added Figure 1 to describe the situation of the disaster.

93 – is that a good assumption? I have no information other than what I can imagine such as wood and sediment accumulating in deltas or alluvial fan locations, but what about wood moving into the floodplain and racking on floodplain trees while the sediment is more or less in equilibrium? I would like to see a bit of a review on how sediment deposits in watersheds. Wood can travel long distances in floods, much longer than sediment. How do you resolve the different virtual velocities of these two components?

Answer;

This concept is based on the situation that considerable amount of sediment and large wood were deposited where the sediment transport capacity was suddenly decreased in the flow direction in places such as fan topography formed by debris flow, as shown in the left photo in Figure 1; This explanation was added in section 1. In addition, to show the flood disaster and wood deposition clearer, we added Figure 1.

118- I wonder if this function could be applied to more than just bridges.

Answer;

We modified relevant descriptions; such as 'structures such as bridges'.

147 – figure 6 is not effective. I have a hard time understanding how to see the lateral distribution effect on Figure 6.

Answer;

We are sorry for the poor figure in the original manuscript. We improved the figure and captions (New Figure 7). Location of the valley is added in Figure 4.

159 – sentence beginning with "As for the water discharge…" is not clear. The authors need to explain in more detail. Clarify what is methods and what the results. A discussion point

of comparing with other vaguely cited reports is also presented very quickly. What exactly is presented in other reports. And what is the discharge?

Answer;

We described this part in more detail. Since there is no hydrological record in the Akatani River basin, the model parameters were validated using the Kagetsu river basin data, which locates east of the study basin.

175 – the section on boundary conditions is critical for the success of the model but is presented very quickly, citing mainly another paper Harada et al 2022 (Entrainment of bed sediment composed of very fine material in ESPL). It is possible that the authors are citing Harada and Egashira 2022, which has a more relevant title (Methods to analyse flood flow with a huge amount of sediment and driftwood), but this publication is in JSCE in Japanese and so will not be accessible to most readers. I think more detail is needed on these upstream boundary conditions given that they are models in themselves rather than measured time series. The authors tend to say things like "The occurrence of landslide, debris flow, and large wood transport induced by the landslide on the hill slope are evaluated based on the method of Yamazaki et al. (2019)," which seems to assume that we are all familiar with these methods. I at least am not and will need more detail on the basic steps, even if further details may be in the 2019 citation. In another sentence the authors say "in which a section that includes the upstream confluence and excludes the downstream confluence point is designated as the unit channel, and the sediment and large wood runoff for the entire basin is predicted by allocating the unit channels in series and parallel." I'm sorry but this is too vague.

Answer;

We described this part (section 3.2) more detail so that the readers could understand the outline of the process to estimate the time series of water, sediment, and large wood discharged from the basin. Indeed, we understand that more detail descriptions and discussions, such as sensitivity analysis or statistical discussions might be required, since the purpose of this study is to describe the 'method to evaluate large wood behavior in terms of convection equation', we would like to explain the process here only to the extent that allows the reader to understand the process.

182 – a lot of calibration in hydraulics is done with the roughness coefficient alone. It would be worth commenting on whether you could calibrate your model with just n. Is 0.03 justified or is it just because it is a typical value?

Answer;

The roughness coefficient is set as 0.03 for the entire computation domain, which was determined so that the flood marks would match the computation results. This sentence was added in the manuscript.

180 – earlier you said the average slope was 1/70. Which is it or over what reaches do these averages apply?

Answer;

The bed slope in section 3.1 is the bed slope for the entire Akatani River channel (1/70), and the slope in section 3.3 is bed slope of the computational domain within the 3.5km (1/120). We clarified these points.

191 – I might have missed this, but where are the cases described? I see some description in the Discussion (lines 215), but this should have been clearly presented in the methods.

Answer;

The case descriptions were added in the manuscript.

230 – This sentence seems to be the justification for assuming that wood and sediment behave the same in terms of erosion and deposition. I found this assumption questionable given the different properties of the wood and sediment, with the key difference being the density relative to water. Wood travels much faster and at the water surface, interacting more with the banks rather than the sedimentary bedforms, at least at high flow conditions. Figure 10, for example shows that wood deposits far from the main channels, with only minor correlation with the bridges. I don't think that the correlation between the observed and computed figures is strong enough to say that the assumption is correct. The wood tends to accumulate far from the thalweg, but the bridges are the highest modelled accumulation points because of the equations used and assuming values of 1.0 with respect to the amount of wood trapped.

Answer;

With regard to the reviewer's point; "Wood travels much faster and at the water surface, interacting more with the banks rather than the sedimentary bedforms, at least at high flow conditions. " In our model, as shown in Equations (14)-(17) and Figure 3, if the ratio of water depth and diameter of a wood piece exceeds 2, large wood is not deposited on the riverbed. In other words, if the water depth is deep enough, large wood pieces are not accumulated on the riverbed and flows downstream with the flow.

The difference between the left and right sides of Figure-11, i.e., the correspondence between the observed and calculated results of driftwood, has been added in the manuscript as follows;

With regard to the spatial distribution of large wood deposition, the difference between the left and right figures of Figure 11 shows the correspondence between the observation and calculation results of large wood. In area (b), where large wood tends to accumulate near the bridge, the observed and calculated results correspond to some extent. On the other hand, in area (a), the observation results show that large wood is deposited far from the original river channel, while the calculation results show that large wood is deposited close to the original river channel, i.e., the right side of the white dotted recutanglar. In this area, the flow that is separated from the main flow becomes an eddy and deposits suspended sediment and large wood at far from the original river channel, whereas the phenomena is not well repdoduced in the computation due to the issue of grid scale. Since the present method assume the large wood deposition occurs where sediment deposits as described in equations (14) and (15), the reproductivity of bed deformation greatly influences the results of large wood deposition.

Figure 5 – repetition of colors and different units are not clear at all. What does 3.5km-2 mean vs 3.5 km-1? Are these repetitions at the same location? Why are two needed? The color scheme for the size distribution is not helpful and overall the figure quality is poor – location diagram is a google earth photo with pins dropped at the approximate locations.
Answer; We reduced the number of lines and made both figures much clearer. (New Figure 6)

Figure 6 – wood length distribution legend not clear. What areas are used? What does inside the valley mean? Can these be put on a map somewhere?
Answer; We improved the figure and captions (New Figure 7). Location of the valley is added in Figure 4.

Figure 8 – best to reiterate what the difference is between cases 1, 2 and 3 in caption
Answer; We added the cases in cation. (New Figure 9)

Figure 10 – legend should say 'wood pieces' instead of 'woods'
Answer; The legend was improved. (New Figure 11)

Figure 11 – figure quality poor due to positions of the axis labels and the low resolution of the graphic. Hopefully vector versions of these plots will be provided.
Answer; We improved the figure qualities. (New Figure 12)

Comment from Anonymous Referee #2

Dear authors,

I have read your ms “Method to evaluate large wood behavior in terms of convection equation associated with sediment erosion and deposition” with great interest. The prediction of wood dynamics (the term “driftwood” should be used for wood elements floating in lakes and oceans only, not in rivers) during large floods is undoubtedly crucial in many regions worldwide, and available models are still few and very often untested against real events.

Having said this, I do not think the model proposed in the ms represents a step forward in our ability to predict wood transport, and thus manage potential wood hazards. Although the effort made by the authors in trying to express mathematically wood transport processes in Eulerian terms is understandable, it is well known on one hand that a model should simplify the reality only up to the point that dominant driving factors are still captured. On the other hand, a model should not be more complex than needed for its purposes. While the authors claim that the model has been successfully validated against the 2017 post-flood surveys, in my opinion this is not the case. The major points of criticism from my side are reported below, and I hope they may help you.

Best wishes

The 2D model is claimed in the introduction to be able to “describe the behavior of large wood based on the convection equation and the storage equation with sediment erosion and deposition to simulate the behavior of numerous numbers of large wood pieces”. If I am not wrong, the present formulation – for both sediment and wood – neglects entirely the bank erosion process. Such process is well known to be the dominant wood supply mechanism in partly-confined and unconfined rivers, and bedload rates may also be greatly influenced by lateral channel migration. Also in the case of the Akatani river bank erosion/channel widening seems (based on Fig. 4) to have been a massive “player” during the flood;
The model to compute the “amount of sediment and large wood inflow from the basin at the upstream boundary of the 2-D analysis” is swiftly presented, with too few details and insights about its plausibility/performance. Uncertainties in the prediction of mass movements processes (location, volumes, connectivity with the channel network) coupled with their wood supply are huge, and there is not track of this in how the upstream boundary conditions have

been later used for the 2D simulations. Furthermore, the forest stand parameters are said to have been "assumed", but in such a relatively large basin area a constant value for them is highly unlikely to be real. Several different wood input scenarios should have been tested at least, integrated with bedload transport scenarios;

The validation of the model is absolutely not convincing, being proposed in a highly qualitative and non-systematic way though the domain. In addition, the arguments brought to support that idea that the model has been successful mostly rely on comparing – again too vaguely, in semi-quantitative terms at best – the flow field and on the deposition pattern in the proximity of the bridges. But this is an "easy win", as for sure bridges are areas where wood was trapped (Dirac delta imposed = 1) and thus flow (increase in flow depth and diversion around the bridge) and sediment transport (deposition) were affected. Therefore, I would say that the model validation suffers from both strong equifinality issues due to its large number of unconstrained parameters and from a tautological argumentation without an accurate, statistically-based accuracy analysis, recalling also the uncertainties regarding the input conditions.

Regarding the practical outcomes of the model proposed, the same conclusions about the role of bridges in the Akatani flood could have been obtained by applying any hydraulic or morphodynamic model with the use of reduced cross-sectional areas at bridges due to expected high wood load. As the authors correctly say, an accurate estimation of the extent to which wood may clog a bridge is crucial for this aim, but this is not incorporated in the proposed model. I was a river manager, I'd certainly use simple, empirically-based rules to include the role of wood and sediment deposition at bridges in robust hydraulic models rather than using a very complex, time-consuming 2D model whose outputs are subject to large epistemic and aleatory uncertainty.

Answer to Anonymous Referee #2

We really appreciate the time and effort you have dedicated to providing insightful feedback for our paper. After perusing the reviewer's remarks, we found that there are several parts in which our intentions have not been communicated. For example, what type of disaster it was and that we are conducting this study to evaluate large wood behavior for such a disaster. In other words, we think your point that "a model should not be more complex than needed for its purposes" is a criticism because the 'needs' and 'purpose' are not well conveyed.

Therefore, in this revision, we have first made significant additions to Section 1 to provide a more detailed description of the disaster. For example, in the Akatani River disaster, it is estimated that approximately 19,500 pieces of large wood were produced by landslide and debris flows, and that large amounts of sediment and large wood were supplied to the river. Thus, an appropriate evaluation of such large wood production, transport, and deposition process is the most fundamental aspect of numerical analysis for this event. The channel winding in Fig.5 took place due to the sediment deposition in the valley bottom, that is also clear from Fig.10, thus we hope the reviewer understands that bank erosion process is not a major factor that the authors intend to discuss.

We also believe that the bank erosion is one of main causes for large wood pieces. Equation (4) normally evaluates bank erosion in which x- and y- components of bed-load rate (q_{bix}, q_{biy}) are evaluated by the bed shear stress and the velocity in the vicinity of the bed where the secondary currents are produced due to curvature of stream lines, and thus the source of large wood is evaluated by Eq. (16) and (17) in case erosion occurs. Thank you for your valuable comments in this regard.

In response to the comment about the lack of explanation of the upstream end boundary conditions, we have made significant additions to Chapter 3.2. Indeed, we would like to write so much in chapter 3.2 that it should be a stand-alone paper, but that would make the paper too large. Therefore, we revised the chapter so that it makes some sense only in this paper. Regarding the point, 'Several different wood input scenarios should have been tested at least, integrated with bedload transport scenarios', please understand that we have performed parameter calibration to match the observed collapse area and large wood runoff estimation, as we have added in chapter 3.2.

We understand that your criticisms about the validation of the model, especially the lack of statistical discussion, that is what we need to pursue in the future. On the other hand, we hope you understand that it is not easy to obtain data on such disasters. For example, it is not easy to obtain data on the spatial distribution of large wood deposition after a disaster, because some large wood is buried under sediment. Indeed, purpose of the present paper is to show the concept of convection equation to analyze the large wood behavior and its applicability for the such extreme disaster. We understand that there are issues that need to be addressed in the future, including the large wood capture at the bridge (Dirac delta imposed = 1).

Finally, we appreciate your insightful comments again, and the revised sentences are marked in the revised manuscript.