“Assessment of geomorphic effectiveness of controlled floods in a braided river using a reduced-complexity numerical model”

Ziliani et al.

General Response to reviewers

Dear Editor,

We thank the reviewers for their thoughtful and useful comments. All comments are constructive, they are very useful to clarify and improve some specific aspects of the manuscript. In this rebuttal we address all the reviewer comments: detailed responses are provided in bold below.

Both reviewers pointed out some problems with English: a careful editing of English will be carried out in the revised manuscript.

Regards

Nicola Surian

February 17, 2020

Response to reviewer 1 (Anonymous)

The authors apply a 2D reduced complexity morphodynamic model to a 7 km-long reach of the braided Piave River. Their goal is to establish that the model credibly represents changes in channel morphology and to ask whether artificial floods might change the future morphology. The applied interest is maintaining channel width and braiding complexity on a river that is progressively narrowing and simplifying due to water management.

The paper is generally well organized and written clearly, with an appropriate amount of documentation. There are about 25-30 small examples in which a careful copy editor is needed to correct English phrasing. These errors rarely produce ambiguity, but should be corrected. I did not have time to mark them myself.

The authors do a good job explaining the model and testing its suitability. They approach the difficult issue of matching model prediction and reality with care. My comments are intended to suggest additional means to explain, evaluate, and justify the model results. I think they should be addressed by the authors, although they do not all need to be acted on.

(1) My first look at the test hydrographs (Figure 7.c, d, e) suggested to me that little difference in predicted channel morphology should be anticipated. The controlled floods in Scenarios 2 and 3 are too small and the controlled floods in Scenario 4 are too infrequent (and no larger than natural floods). Presumably the flood scenarios chosen are as large as can be released given the water infrastructure in the basin. Hence, the (not too surprising) result is that the modest or infrequent floods that are feasible are not sufficient to produce significant changes in the forecast channel morphology.

Thanks for this comment. As suggested by the reviewer, we did not expect major changes in channel morphology but, considering the controlled floods used in the three Scenarios, some significant changes could be expected. As already explained in the manuscript, the controlled floods are feasible, that is taking into account the water infrastructure in the basin. On the other hand, it is worth noting that the floods in Scenarios 2 and 3 are not so small (these are formative discharges and are released for 5 days) (Table 2). We agree that controlled floods in Scenario 4 are infrequent, but those floods are quite large (recurrence interval = 5 years) and released for 1 day (Table 2). Therefore, some effects on channel morphology (i.e. some geomorphic recovery) could be expected. The results show that any significant recovery took place. A main outcome of this paper, that could not be anticipated when we started this research, is that controlled floods may have any significant effects in a strongly regulated river, specifically if formative discharges have been strongly altered. In terms of changes in the manuscript, we will carry out small changes in the Discussion (Section 5.1) and in the Conclusions.

(2) An interesting way to present the results would be in terms of 'limits of prediction'. That is, conduct multiple runs driven by small changes in parameter or initial conditions or in the sequencing of floods, in order to show how variable the results would be given uncertainty of the input. I would guess that the range of predicted width and braiding index (Figure 7.a, b) would comfortably encompass the predictions from the different scenarios, indicating that the model is unable to demonstrate that the available floods would produce different morphologies.

We agree with the reviewer, a sensitivity analysis would be needed for better assessment of the results and associated uncertainty. On the other hand, since a comprehensive testing of the model is very complex (this is also pointed out by reviewer 2 "…to evaluate/test a morphodynamic

model (which is a complex and far from straightforward exercise)…") and we could rely upon a sensitivity analysis that we carried out in a similar river (Ziliani et al, 2013, JGR), the sensitivity analysis was out of the scopes of this work. That said, we carried out a calibration (Section 4.3) which shows a very good performance of the model, specifically as for estimate of channel width and braiding index.

(3) I do wonder whether the model is able to predict larger widths. Is the model capable of predicting a width of, say, 430 m, as observed in 1970? I realize that flows up to 1970 are outside of the calibration range, but I would be concerned about whether the apparently firm upper bound on width of 350 m (Figure 7.a) is somehow an artifact of the model.

Thanks for this comment. We think that the model should be able to simulate larger widths and that the lack of very large floods in the scenarios prevented further widening (i.e. channel widths larger than 350 m). To confirm our hypothesis, we will run a new simulation which will include a very large flood (e.g. 50 yr flood). This will be done before re-submitting the revised version of the manuscript.

(4) The test for sediment transport rate is quite weak: the authors find that the computed transport rates are within typical range for such gravel-bed braided rivers. A more sensitive test would be to evaluate how the bed grain size changes over time. You specify an initial grain size - does that grain size shift dramatically over the course of the model run?

Thanks for pointing out this. We analyzed grain size changes, specifically we compare D50 of bed sediment at the beginning (i.e. 24.9 mm) and at the end of each Scenario. The D50 changes are small, since D50 is 22.7, 24.3, 23.6 and 23.1 respectively at the end of Scenario 1, 2, 3 and 4. In the revised manuscript this information will be included in Section 5.2 ("Assessment of CAESAR-LISFLOOD performance").

(5) River are a combination of sediment-feed and sediment-recirculating systems. I suspect that the model results are sensitive to this choice. The problem, of course, is specifying an upstream sediment boundary condition. I would be interested in learning how an increase in sediment supply changes the predicted channel morphology. Perhaps that is beyond the scope of the paper, although the authors do mention that sediment mining was practiced and then halted. Model runs with and without substantial sediment removals would certainly be interesting!

We agree, in general for such dynamic systems it is crucial to take into account also possible changes in sediment supply. In this case study sediment supply was kept constant for two reasons. First, including

changes in sediment supply would imply to carry out several other Scenarios, adding complexity to the modelling and to the overall work. Second, and most importantly, there is evidence that the study sector is not undergoing significant vertical changes (i.e. incision or aggradation) over the recent period (see line 129 of the manuscript). Besides, it is not likely that sediment mining will be carried out in the study sector in the near future. For such reasons, the sediment recirculation option of the model (i.e. sediment transport equilibrium condition) was adopted for this study.

Response to reviewer 2 (Tom Coulthard)

This is a really neat study looking at how a reduced complexity morphodynamic model can be used to investigate how the hydrological perturbation of flows within a managed braided river affect the morphology. Ultimately, it shows how controlled releases of larger flows have some but not significant impacts on channel widths and depths within the reach studied.

In doing so it also provides an excellent opportunity to evaluate/test a morphodynamic model (which is a complex and far from straightforward exercise) demonstrating how such methods can be used as management tools in such environment to answer questions about hydro-geomorphic interactions.

The paper is well written, produced and structured. As per R1, there are several minor grammatical/typo mistakes that can be picked up in proof reading if the paper progresses.

Some specific comments and suggestions for further literature that has not been cited (some has only just come out) are provided below.

- 65 Impellent?
  We will change this adjective

- 70 A reference to Larsen et al https://doi.org/10.1002/2014EO320001 might be useful in the RCM description section here.
  The suggested paper will be cited, as it properly emphasizes the exploratory purpose of RCMs application in fluvial geomorphology as well as in other complex Earth and environmental systems.

- 230-236 - felt a bit clunky and repetetive - might be worth having a closer look at this section. Also there have been a series of CL papers and studies since 2013 that might be useful for the paper to cite here as well. There are others but two more recent ones CL Sensitivity analysis paper: https://www.geosci-modeldev.net/11/4873/2018/ Calibrating valley floor re-working in CL. Feeney et al., 2020 https://doi.org/10.1002/esp.4804.

Thanks for pointing out these two papers which represent the most recent applications of the CL model. Skinner et al. (2018) is focused on a global sensitivity analysis applied to CL model (catchment mode application) used as Landscape Evolution Model. Feeney et al. (2020) focuses on the effectiveness of CL model in reproducing historical channel lateral erosion and modelling floodplain turnover (in single thread sinuous o meandering reaches, locally wandering).
We will revise the last part of section 3.3. (L 230-236), including these suggested papers.

- 237 Section 3.4. The two references above are also really relevant for this section there have been more thoughts and studies on validation/calibration methods. Both compliment what you are doing here I think.

  We agree with the Reviewer. Both papers will be cited in the revised version of the manuscript, since they are complementary to our work (they differ from our case study in terms of spatial scale of the application – in Skinner et al., 2018; temporal scale and morphology of the river reaches – in Feeney et al., 2020).