Answer to RC2: Comment on esurf-2021-91 from Clément Misset

June 28, 2022

We wish to thank the referee for his careful evaluation of the manuscript. Please find below the detailed answers. The reviewer's comments are shown in bold and some modifications of the manuscript are emphasized in blue.

General comments

This manuscript presents the 2D numerical modelling of morphodynamics in a braided river reach during a large flood event. To develop and test the 2D model, it uses observed morphological changes (DoD obtained from Lidar data) as well as hydrological data and field measurements. This is thus a nice dataset. The manuscript is generally well written and addresses important questions for both researchers and practitioners, regarding the modeling of morphodynamics in braided systems. Several friction laws and bedload transport formulae are tested in the numerical model in order to evaluate the most relevant modeling approach. Criteria to evaluate the model performance in such context are discussed as well as future improvements. I think this work is of interest for the reader of Earth Surface Dynamics.

I have however several concerns, that I suggest to consider in a revised version of the manuscript:

1. **I think some relevant literature is missing, in particular regarding braided morphodynamics modeling and criteria that have already been proposed to evaluate 2D morphodynamics models (see detail comments).**

2. **More details could be added when presenting the studied site as well as some of the model parameters, for the reader to better understand the setting and modeling choices (see detail comments).**

3. **Concerning the sediment transport deposition partitioning, I have a major concern: can the historical partitioning observed with a lake configuration be used for the braiding setting? I guess this is not the case. Using the same values (8-16% of bedload) should be discussed regarding the hydrodynamics observed/modeled, typical values observed in previous works, etc. This assumption seems to be critical, in particular for the model evaluation.**

4. **The choice (critical) of the upstream boundary condition for bedload flux would beneficiate to be better argued.**

5. **Concerning the performance criteria, I think the paper would greatly beneficiate to include other criteria adapted to braided rivers. The statistical distribution of erosion and disposition have for instance been used in a similar study by Williams et al. (2016b). There are also several braiding index that have been used by Rifai et al. (2014). These aspects are already discussed in the current manuscript, but I think using more appropriate index could significantly improve the analysis.**

6. **It could be considered to add a discussion section, to better distinguish the results description and their interpretation.**

To sum up, I think this work is original and will be of great interest for geomorphologists. I however think that substantial revision could be made to improve the manuscript regarding the previously mentioned aspects.

1. We will add relevant literature as proposed by the reviewer. The details are given below in several detailed comments (e.g. L65, L415) and in the references.

2. A better description of the study site including length and slopes will be added in the text. A plot of the longitudinal profiles extracted from the LIDAR DEM of 2016 and 2019 will be added in the description part. The figure that we intend to add to the last version of the article is presented below (Fig. 1).

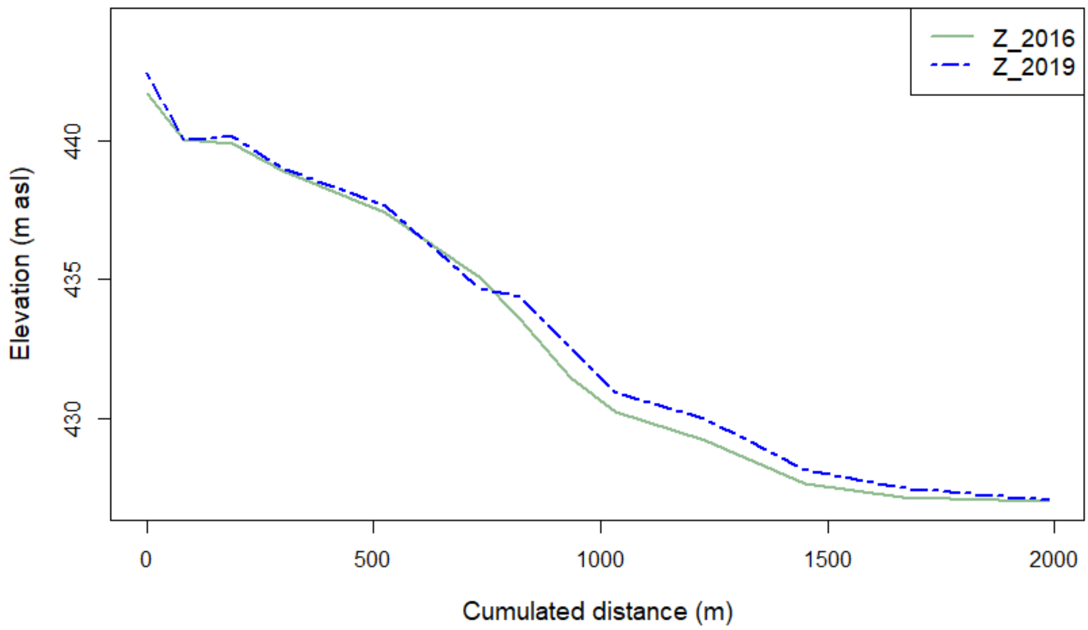

Figure 1: Longitudinal profile evolution following the flood of 2018.

3. This question is very relevant and we understand the concern about using historical dredging data to estimate the part of bedload transport within the study area. However, we consider that the site configuration is intermediate between a lake and a braided river since the slope is lower than the equilibrium one because of the downstream weir. This decelerated flow is observed in the major part of the domain where sediment transport is analysed in detail (between the two weirs). As a consequence, if we take the value for the lake, we observe that the part of the bedload is close to the upper interval limit because the hydrodynamic forces are actually larger than within the lake. The conclusions about bedload transports as a function of the models are not influenced by this assumptions. Concerning comparison with experiments, the analysis is completed by profile in the following.

4. The upstream condition is indeed a crucial point. As mentioned, imposing solid discharge from a reach-averaged equation was difficult and generated numerical instabilities at the level of the boundary cells with other kinds of solid boundary conditions. Nevertheless, the area of interest is between the two weirs (between 500 and 2000 m in figure 10). In this part, the bed evolution is less influenced by models than the upstream part. Then it can be assumed that the solid upstream boundary condition has low influence in the area of interest. The upstream part of the domain is used to create a more realistic solid discharge.

Clarifications will be added to the text.
The upstream boundary condition is located sufficiently far from the area of interest to influence its dynamics directly.

5. As suggested, we will include the statistical distribution of erosion and deposition used by Williams et al. (2016b) (See Fig. 2 upstream the LDG and Fig. 3 within the LDG). Compared to the measurements, it can be seen that some models reproduce the spatial distribution of deposited or eroded areas better than others, this will be further discussed in the article to improve the analysis.

6. A discussion section will be added in the article.

Detail Comments

Introduction: I suggest to better define "complex" morphology. I also suggest stating sooner and more clearly the main objectives of the paper, for the reader to understand why a numerical model is developed.
By complex morphology, we mean a river with many channels and very heterogeneous grain-size distribution. It is not a single channel with uniform materials. To better present the interest of numerical model, we will add clarifications at the beginning of the introduction:
Numerical models allows taking into account complex geometry with several braid river but also various class of sediment. Generally they are necessary in case of several physical models have to be considered. For instance, they provide velocity, suspended concentration of grain size transported which has to be known for ecological purpose. For flood impact forecast, they are able to estimate time scale of erosion or deposition. They can also evaluate morphological evolution in area where the expertise is lacking as close to hydraulic structure with specific design.

L19: Sentence is not clear, are you citing Rickenmann et al. (2016) or Reisenbüchler et al., 2019? Consider rewrite the sentence
"They showed" will be replaced with Reisenbüchler et al. (2020) showed.

L27: the term "condition" is vague, be more specific. Natural hazards have been already discussed in previous lines; I suggest removing it from this sentence
We will precise the term and remove the redundant following sentence.
Besides safety issues, bedload transport, combined with water discharge, is considered a fundamental driver of river morphodynamics and risks of overflowing.

L36: I suggest adding that hydrodynamics is also driven by morphological changes
We will add a sentence that underlines the back influence of morphology on hydrodynamics.
Simultaneously, the morphological modifications have then an influence on the hydrodynamic simulation.

L41: using valid sediment transport formulae (friction law, etc.) is not only needed to introduce reliable boundary conditions but to develop realistic modeling "inside" the model domain (at the grid scale).
We will rewrite the sentence which was confusing about the need to have robust modelling of the physical phenomena.
To have a physically realistic simulation, it is necessary to provide the model with realistic bedload transport rates to introduce reliable boundary conditions and physical modelling within the study area extent.

L65: I think there is some relevant literature about braided morphodynamics modeling that is missing. I suggest considering Williams et al. (2013), Williams et al. (2016a), Williams et al. (2016b), Rifai et al. (2014), Gonzales de Linares et al. (2021). Comparison with these works would also be relevant in the discussion

section.
We will add the proposed literature, which completes usefully the previous ones.
For braided morphodynamics modelling, the model performance can be provided by a specific indicator a the scale of the area of interest (Williams et al. (2013), Williams et al. (2016a), Williams et al. (2016b), Rifai et al. (2014), Gonzales de Linares et al. (2021).

Study area description: I think it would be of interest for the reader to add a more detailed description of the site, in particular, including a river profile showing slopes, the tributary, solid material input, etc. If available, I suggest to use a hillshade map of the catchment in Fig.1 (instead of the grey areas) to better show the relief and the mountainous environment. Maybe also add pictures illustrating the upstream main tributary morphology?
A better description of the study site including length and slopes will be added in the text. A plot of the longitudinal profiles extracted from the LIDAR DEM of 2016 and 2019 will be plotted in the description part (Fig. 1).

Fig2: I suggest writing directly on the picture the dates and mention "before the 2013 flood", "after the 2013 flood".
This will be taken into account in the last version of the article.

L104: "compared to 90 m3/s annually in the same period" -¿ do you mean: compared to the monthly averaged discharge of 90m3/s?
Yes exactly. The sentence will be modified as proposed.
compared to the monthly averaged discharge of 90 m^3/s.

2.4 Restoration implications: I have a concern regarding the model aim. I am not convinced that once calibrated on a rare event such a the 2013 event, the model will automatically be applicable for long term modeling, in particular for low magnitude events that would likely contribute to transport the stored sediments from the LDG to the downstream reaches in case the weirs are removed.
The calibration was made on the 2018 event, which is a 10 year return period episode and not on the 2013 event, which is more extreme. We also chose this event because we consider that it can be more representative of the sediment transport phenomena that we want to consider for the restoration measures.

L128: I would add something like "Thus, to help decision makers, a hydro-morphological 2D model..."
This sentence will be added in lines 128-129.

3.2.1 Friction laws: specify that the friction law is not coupled with the bed surface grain size (which is constant because fractional transport is not considered in the morphodynamical modeling).
The friction law is indeed not coupled with the transport model. The value of D_{84} is not calibrated but taken from measurement in the field. We will add the following sentence:
The value of D_{84} is directly obtained thanks to the grain-size measurements done at the bed surface in the LDG area (see part 4.1.1).

L170: I wouldn't say "The friction coefficient for the Ferguson (2007) law is the D84", I would say something like "the Ferguson law uses the D84 as a proxy of the bed roughness" or remove this sentence
We decided to remove the sentence because the information appears clearly in equation (3).

L185: I found the sentence not clear, K/K' is used for shear stress partitioning if I'm right. Do you use this shear stress partitioning in your 2D modeling? According to Gonzales de Linares et al. (2020, 2021), such correction might not be relevant in massive bedload transport. The recking formula: I have a major concern here. You use a 1D "morphological" formula that takes implicitly into account the cross-section averaging (variability of shear stress, grain size distribution, etc.) to deal with non-linearity effect of bedload transport. In your 2D model, the shear stress is spatially discretized so that the Shield number used to calculate the bedload flux is a "local" Shield number and the bedload transport formula does not need to take into account cross section averaging. I suggest considering the following papers: Recking (2013) and Recking et al. (2016).

The MPM law was used with the correction because the conclusion of Gonzales et al. (2020, 2021) was not known during the simulation studies. But this will be mentioned in the discussion. The Recking formula that has been implemented is a version compatible with 2D calculation. It will be added in the text.

We used the version of this formula compatible with 2D calculation and local data (Recking et al., 2016a). It can be written as follows (Eq. 1):

$$q_b^* = \frac{q_b}{\rho_s\sqrt{g(s-1)D_{84}^3}} = 14\frac{\tau^{*\ 2.5}}{1+\left(\frac{\tau_m^*}{\tau^*}\right)^{10}} \tag{1}$$

q_b^* $[-]$ is a dimensionless bedload discharge, q_b $[kgs^{-1}m^{-1}]$ is the unit bedload discharge per unit width, $s = \rho_s/\rho$ is the specific gravity, and g the gravity acceleration. τ^* $[-]$ is the Shield number, calculated from the diameter D: $\tau* = \frac{\tau}{g(\rho_s-\rho)D}$ with τ $[N/m^2]$ the shear stress. Here the calculations were made using D_{84} as the grain diameter. The parameter τ_m^* is a mobility term that defines the transition between partial transport ($\tau^* < \tau_m^*$) and full mobility ($\tau^* > \tau_m^*$) (Recking et al., 2016a). The Recking formula was calibrated on field data ($\tau^* < \tau_m^*$) and laboratory data ($\tau^* > \tau_m^*$). It is the value of τ_m^* that gives its shape to the model. Therefore the value of τ_m^* strongly impacts the result, and its determination is difficult, especially for mountain streams. Ideally it should be based on measurements. Failing that, the available data suggest that an estimate is possible using Eq. 2 (Recking et al., 2016a).

$$\tau_m^* = 0.26S^{0.3} \tag{2}$$

3.2 Sediment transport and bed evolution module: Could you specify if an avalanche mode (Slope sliding) is used and what are the parameter considered (angle of repose)? Same question regarding the deviation of sediment transport on transverse slopes? Both could have significant effect on the morphological modelling, it is thus of interest for the reader to know which parameters were used.

The slope sliding effect was of course tested to analyse its influence on sediment transport. The two available formulae were considered. However, they both exagerated sediment depositions between the two weirs. Hence, this parameter was not considered in the presented results. Besides, as the "angle of repose" option is only activated if one of the two slope sliding equations is used, it was not considered in the presented results but was analysed during the sensitivity analysis that we performed before the selection of the most relevant parameters.

L223-224: could you provide the point density of the Lidar and the raster resolution of the used DEMs? Could you specify here if bathymetric data for the low flow channels were available?

We will specify the resolution of the DEM in the last version of the article. The planimetric resolution is 1 meter. No reliable bathymetric data was available for 2019, thus we prefered to apply the same methodology for 2016 and 2019. The two DEMs were surveyed during very low flow periods, which reduces the uncertainties concerning the bathymetry of the water area.

L235: It is not clear for me how you estimate the bedload fraction? Do you have GSD measurement of the dredging? This should be clarified. The range 8-16% seams possible but much wider range could be considered, see for instance Turowski et al. (2010). I also wonder if you can consider the same partitioning between the pre and post flood situation: Fines will likely deposit more easily in a lake (even if they will deposit in the braiding situation). Note that the sediment transport partitioning might also evolved with the event return period and the flow/sediment supply conditions so that dredging might not be fully representative of a specific event. The following papers could be of interest to consider a gravel matrix fine fraction commonly observed in gravel bedded stream: Mueller and Pitlick (2013), Navratil et al. (2010), Misset et al, (2020)
The dredging data were used to estimate the bedload fraction. However, no GSD measurements of the samples were available. This will be clarified in the last version of the article. The model presented in the article considers only one sediment diameter: D_{84}. Therefore, it does not allow to check the interesting remark regarding the evolution during the flood. To overcome this drawback, simulations on the updated sediment transport module GAIA, developed to handle the grain-size distribution issue better, are currently in progress.

4.1.2 Input hydrograph: do you have points with liquid discharge measurement to calibrate/validate the model? This should be specified.
We will add the following information to explain the use of the hydrological model MARINE.
This model has been calibrated based on the available observed discharges at three stations: the Gave de Cauterets, the Gave de Gavarnie and the Gave de Pau after the confluence with the Gave d'Azun. 6 events extracted from these observed time-series allowed calibrating the model with a good confidence.

Fig5. It is not clear for me if the large model uses a fixed bed? I suggest to add a title above each mesh (large, finer mesh).
It considers non erodible areas but the bed is not fixed. We will add the following information:
For the simulation with both meshes, the sediment transport model was used.

L277: the equilibrium load upstream condition: Is this relevant? Were the upstream profile and cross section stable? Did you compare these upstream fluxes with an averaged 1D formula? This hypothesis is probably critical for the modeling of the event.
We agree with these comments that are very relevant. The simulation in the first meters upstream is not considered to be relevant as the simulated fluxes do not match the ones estimated with the 1D reach averaged formula. However, when we use the larger model we see that the bed evolution is not important at the upstream boundary of the smaller mesh (see also response to general comments).

L285: I suggest specifying that under the hydrodynamics calibration conditions, morphological changes and bedload transport were limited. If bathymetric data were not available, this can probably lead to a non-negligeable uncertainty on the water surface elevation, which can be considered acceptable compared to other uncertainties, this need to be specified. Also, did you vary the D84 considered in the Ferguson friction law? Or did you use the measured value?
These relevant remarks will be integrated in the last version of the manuscript. The uncertainty

due to the lack of bathymetry can be estimated considering the water depth during the LIDAR survey which was approximately the same for both DEMs. This will be specified in the last version of the article.
We didn't vary the D_{84}; we only used the measured value.

L305-306: maybe use the term DoD (DEM of differences)?
The term DoD will be used.

4.4 Performance evaluation: Braiding and morphological changes in braided rivers are somewhat stochastic processes. Is it thus relevant to use single long profile/cross sections and the Brier Skill Score objective function? I mean, your model can be considered relevant in reproducing the morphodynamics of the studied reach as it reproduces well some general morphological evolution (for instance the braiding index, the number of active channels, the statistical distribution of erosion and deposition, the average long profile, etc.) but the objective function can lead to a bad score (you do not have exactly the same DEM evolution).
We totally agree with this remark and we tried to criticize the use of the BSS and long profile evolution in the discussion section. Besides, since morphological changes are indeed a stochastic phenomenon in braided rivers, we considered that a volumetric analysis in which we compare the deposition volumes might me more relevant and sufficient for the purpose of our study. We will add more precision in the discussion section and introduce other more relevant criteria to consider for the analysis of morphological changes in braided rivers (such as the ones that you suggest by Williams et al. (2016b)).

4.4.3 Comparison of the deposited volumes Comparison of the global volumetric budget of the reach is of great interest. I have however the filling that DoD obtained for the 2018 flood includes much more information. You could for instance compare that statistical distribution of erosion/deposition in the reach, is is done in a similar study by Williams et al. (2016b).
Thank you very much for this suggestion. The statistical distribution of erosion/deposition will be analysed upstream the LDG (Fig. 2) and within the LDG (Fig. 3). We observe that the Strickler friction equation has a completely different dynamic and it tends to exagerate sediment depositions compared to the Ferguson formula. These two figures will be added and commented in the last version of the article.

Fig7: I suggest using red for erosion and blue for deposition (commonly used colors for such map), to include a scale bar and to remove the pixel within the uncertainty range of the DoD (by using transparent color so that only significant morphological differences can be seen).
We will modify this figure according to your suggestions.

L349: Friction laws link velocity to depth and roughness
The sentence will be modified.

5.1 General visual comparison of eroded and deposited areas: according to a previous comment, you are comparing visually the erosion and deposition distribution depending on the friction law and transport formula used: comparing these distributions could strengthen this analysis. Another aspect that could be discussed regarding the effect of the friction law is the use of spatially and temporally constant roughness parameters (n or D84). It is likely that these parameters are not constant in both space and time the braiding reach, which can explain some of the differences between modeled and measured evolutions.
Indeed, in this section we intended to compare the visual erosion and distribution depending on

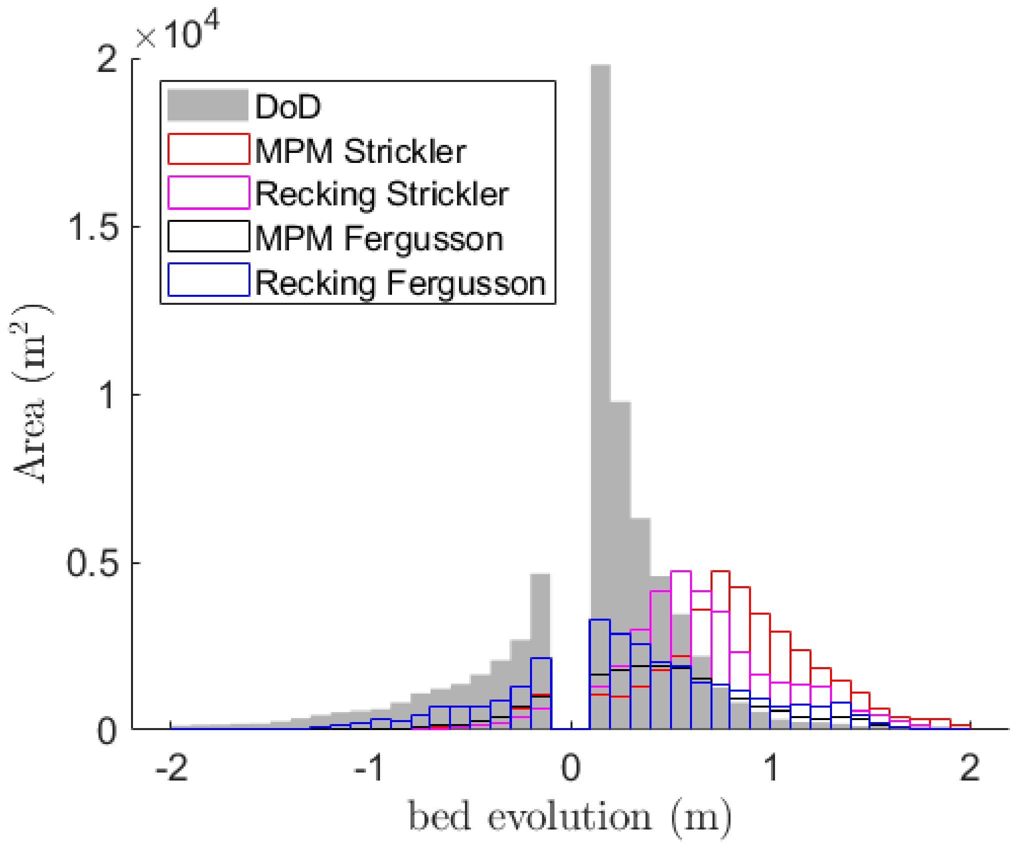

Figure 2: Statistical distribution of erosion/deposition observed with the DoD compared with the simulated evolution upstream the LDG

both the friction law and the sediment transport equation.
We will add a comment regarding the effect of the roughness parameters to better discuss the impact of uniform and constant K or D_{84}.
In line 380 : Besides, it is likely that the roughness parameters used by the two considered friction laws (K or D_{84}) are not constant in both space and time. This can also explain some of the differences between modeled and measured evolutions.

L372: If I understand, the model seems to be not fully relevant in the downstream part of the reach, as almost no evolution is modeled. I agree that such model, already complex, doesn't take into account all the complexity of such braided system. I however wonder if this disagreement is only due to the fact that suspended load is not modeled. Do you have observations showing that the downstream part is mainly driven by suspended load processes? Based on aerial photograph, the downstream part of the reach seems to be at least partially composed of gravel bars? I suggest moving this discussion of the results in the discussion section.
This part of the reach is mainly affected by suspended load even if we can see some gravel in aerial photos. In fact we performed geotechnical surveys (- 4m) in this area to analyse the composition of the deposited materials and gravel is only observed within the first 20 cm. Once this surface layer is passed we only observed very fine and cohesive sediments such as silt. This is why we supposed that this part of the reach is mostly affected by suspended load.
We modified these two sentences to explain our assumptions:
This can be due to the fact that many factors are not considered in the model such as the consideration of the whole grain-size distribution for bedload.
and the following one
The morphological evolution within this section is thus, for the most part, probably due to suspended load, not considered in our model.

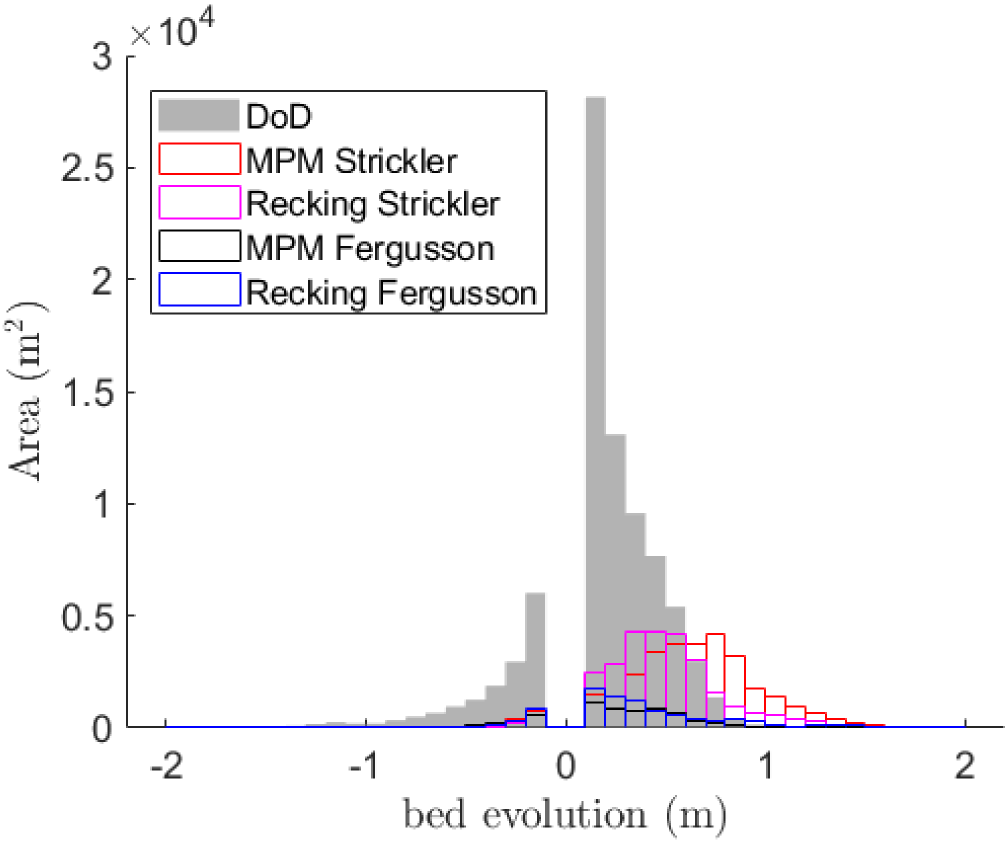

Figure 3: Statistical distribution of erosion/deposition observed with the DoD compared with the simulated evolution within the LDG

Fig.8 and 9: I suggest adding on the map "observed changes" and the names of the transport formula and friction law.
This will be added in the figures.

5.2 Longitudinal profiles and cross-section comparison: I wonder if comparing long profile in braided morphology is relevant? Maybe, using an "averaged" long profile to capture the general trend and being less sensitive the the active channel position would be a better option? I also suggest to move from this section all sentences discussing this specific point and the relevance of the BSS index.
To be consistent with the other part we did not use transversal averaging to analyse local evolution. Obviously, this method does not give an exhaustive view of bed modification. But in this first study, we considered that volume analysis can provide sufficient results.
However the addition of the statistical distribution of erosion and disposition can also help and will be added (see general comment n°5 about the performance criteria and Fig. 2 and 3).

L415: Can we consider the deposited bedload fraction is similar between the 2018 braiding situation and the pre 2013 flood situation with a lake? I have the feeling that the deposited suspended load fraction would be higher in a lake configuration. Were the hydrodynamic conditions low enough (backwater effect, low velocities, low shear stress, etc.) during the 2018 flood so that more than 80% of the deposited volume corresponds to suspended particles? If it is not the case, this fraction seems to be really high. I suggest comparing it with previous works on fine particle stocks/content in gravel bedded streams (see Mueller and Pitlick (2013), Navratil et al. (2010), Misset et al, (2020)).
This point has been discussed in a previous comment. Thank you for the additional references that will be added in the last version of the article.

References

Gonzales de Linares, M., Mano, V., Piton, G., and Recking, A.: Modelling of massive bedload deposition in a debris basin: cross comparison between numerical and small scale modelling, in: RiverFlow 2020, edited by et al., U., Proceedings of the 10th Conference on Fluvial Hydraulics, Delft, Netherlands, URL `https://hal.archives-ouvertes.fr/hal-02935173`, 2020.

Gonzales de Linares, M., Ronzani, F., Recking, A., Mano, V., and Piton, G.: Coupling Surface Grain-Size and Friction for Realistic 2D Modelling of Channel Dynamics on Massive Bedload Deposition, in: Conference: SimHydro 2021: Models for complex and global water issues - Practices and expectations, vol. 30 of *Actes de la conférence SimHydro 2021*, pp. 1–10, Sophia-Antipolis, France, URL `https://hal.archives-ouvertes.fr/hal-03363327`, 2021.

Misset, C., Recking, A., Legout, C., Viana-Bandeira, B., and Poirel, A.: Assessment of fine sediment river bed stocks in seven Alpine catchments, CATENA, 196, 1–14, https://doi.org/10.1016/j.catena.2020.104916, 2021.

Mueller, E. R. and Pitlick, J.: Sediment supply and channel morphology in mountain river systems: 1. Relative importance of lithology, topography, and climate, Journal of Geophysical Research: Earth Surface, 118, 2325–2342, https://doi.org/https://doi.org/10.1002/2013JF002843, 2013.

Navratil, O., Legout, C., Gateuille, D., Esteves, M., and Liébault, F.: Assessment of intermediate fine sediment storage in a braided river reach (southern French Prealps), Hydrological Processes, 24, 1318–1332, https://doi.org/10.1002/hyp.7594, 2010.

Recking, A.: An analysis of nonlinearity effects on bed load transport prediction, Journal of Geophysical Research: Earth Surface, 118, 1264–1281, https://doi.org/https://doi.org/10.1002/jgrf.20090, 2013.

Recking, A., Piton, G., Vazquez-Tarrio, D., and Parker, G.: Quantifying the Morphological Print of Bedload Transport, Earth Surface Processes and Landforms, 41, 809–822, https://doi.org/10.1002/esp.3869, 2016a.

Recking, A., Piton, G., Vazquez-Tarrio, D., and Parker, G.: Quantifying the Morphological Print of Bedload Transport, Earth Surface Processes and Landforms, 41, 809–822, https://doi.org/https://doi.org/10.1002/esp.3869, 2016b.

Reisenbüchler, M., Bui, M. D., Skublics, D., and Rutschmann, P.: Enhancement of a numerical model system for reliably predicting morphological development in the Saalach River, International Journal of River Basin Management, 18, 335–347, https://doi.org/10.1080/15715124.2019.1628034, publisher: Taylor & Francis, 2020.

Rifai, I., Le Bouteiller, C., and Recking, A.: Numerical study of braiding channels formation, in: Telemac Mascaret User Club Conference, pp. 159–167, Grenoble, France, URL `https://hal.inrae.fr/hal-02606139`, 2014.

TUROWSKI, J. M., RICKENMANN, D., and DADSON, S. J.: The partitioning of the total sediment load of a river into suspended load and bedload: a review of empirical data, Sedimentology, 57, 1126–1146, https://doi.org/https://doi.org/10.1111/j.1365-3091.2009.01140.x, 2010.

Williams, R. D., Brasington, J., Hicks, M., Measures, R., Rennie, C. D., and Vericat, D.: Hydraulic validation of two-dimensional simulations of braided river flow with spatially continuous aDcp data, Water Resources Research, 49, 5183–5205, https://doi.org/https://doi.org/10.1002/wrcr.20391, 2013.

Williams, R. D., Brasington, J., and Hicks, D. M.: Numerical Modelling of Braided River Morphodynamics: Review and Future Challenges, Geography Compass, 10, 102–127, https://doi.org/https://doi.org/10.1111/gec3.12260, 2016a.

Williams, R. D., Measures, R., Hicks, D. M., and Brasington, J.: Assessment of a numerical model to reproduce event-scale erosion and deposition distributions in a braided river, Water Resources Research, 52, 6621–6642, https://doi.org/https://doi.org/10.1002/2015WR018491, 2016b.