We thank Referee #2 for his review of our manuscript and making the highly constructive comments and suggestions. We are glad to hear our effort to revise the

Manuscript. The author's response is shown below in **black text**.

First and foremost, is **the lack of proficiency and fluency in the use of the English language and grammar**, which is consistently poor throughout this paper. This makes it very difficult to comprehend the contents of the paper – for example, it is not clear what the methods were, or how sets of parameters used in simulations were obtained/derived. Because of this, it was also not clear how the results were obtained and what they actually represented, and consequently, whether the resulting discussion and conclusions could be substantiated or supported. Overall, while the aims and objectives could be understood, it was not easy to determine if they had been met. Unfortunately, the author's unfamiliarity with the English language meant that too many sentences were variously incomplete, made no sense, or utilized inappropriate or misspelt words.

Admittedly, the English language in this research is not proficient and fluent. We will revise it carefully in the manuscript.

Some comments and suggestions for improvement:

Rather than try to describe the background to the CAESAR model and how it works themselves, I believe the authors could more clearly and succinctly acknowledge this by referencing existing publications which describe this ie. Coulthard et al 2012.

As currently written, descriptions of specific parameters and methods used in this study and the scenarios modelled are poorly described, or not described at all – for example:

As C-L users, we intended to introduce the background of the C-L and the relevant parameters' definitions to understand the main theories best.

a table of parameters lists values used in simulations, yet **there is no explanation of how or why the values in the table were selected or used** – for example, those representing vegetation parameters (shear stress, age to maturity, proportion of erosion) - were selected and utilized in model simulations for the scenarios in this study.

We admit that some parameters were not introduced in detail, except for the definitions, such as shear stress, age to maturity, and proportion of erosion.

Some sensitive parameters to the model (Skinner et al., 2018) were referenced from some published research in the same catchment, such as the sediment transport formula, grain size and corresponding proportion and the slope failure threshold (Li et al., 2020; Xie et al., 2018, 2022). In addition, we add the calibration of the components in C-L in the first reply. Other parameters in Table 2 are from the default values recommended by the developers (such as the max erode limit in the erosion/deposition module and the vegetation critical shear stress) in https://sourceforge.net/p/caesar-lisflood/wiki/Home/.

The authors **do not explain why they selected some of the parameters ie why a specific sediment transport equation** was selected. Depending on the sediment transport equation applied, very different model results may occur.

The sediment transport equation was sensitive to the C-L model (Skinner et al., 2018) and we selected the Wilcock and Crowe referenced from published research studied in the same catchment (Li et al., 2020; Xie et al., 2018, 2022). Actually, the Wilcock-Crowe equations are among the more widely used formulae for predicting fractional bed load transport rates in gravel bed streams. They are developed using bed load transport information obtained in laboratory flume experiments with bed material sediments ranging in particle size from 2.83 to 64 mm (Wilcock et al., 2003). Another alternative equation is Einstein-Brown, which is developed by uniform sediment and lightweight materials ranging in sizes from 0.785 to 28.65 mm based on flume data. From the grain sizes in our study area, the Wilcock-Crowe equations would be the better choice.

It is not clear **what rainfall data was used in the simulations** – whether different sets of data were used for different scenarios, or one set was applied across all scenarios. The text about the downscaling rainfall data is simply confusing and does not address this.

We have revised the downscaling method in the third reply. Now we add Table 1 to address the scenarios settings and the input rainfall. We will provide in the revised manuscript at last.

Table 1 The main settings and input for the three scenarios

<table>
<tr><th>Scenario</th><th>Descriptions</th><th>Period</th><th>DEM (resolution)</th><th>Rainfall data</th></tr>
<tr><td>UP</td><td>no anthropogenic intervention</td><td rowspan="3">from January 2011 to January 2013
(3 years)</td><td>UP DEM (10m)
UP bedDEM (10m)</td><td rowspan="2">downscaled hourly precipitation in the period
(lumped)</td></tr>
<tr><td>PP</td><td>the present two blocking dams in the upstream without dredging work</td><td>PP DEM (10m)
PP bedDEM (10m)</td></tr>
<tr><td>EP</td><td>plus vegetation revetments in the source area and levees in the deposit area based on Scenario PP</td><td>EP DEM (10m)
EP bedDEM (10m)</td><td>downscaled hourly precipitation in the period
(spilt)</td></tr>
</table>

As written, it is not clear how results are **obtained or substantiated** from the methods. The authors **make assumptions about the ability of the model to erode that are not supported or substantiated by any evidence**. Specifically, the authors describe how they have attempted to incorporate levees and dams into simulations by simply increasing the elevation in certain areas and not changing other parameters such as particle size. While this reviewer agrees it may temporarily reduce flow, in the longer term, this may well lead to increased erosion in other areas around the sides of the dam / levee.

We have added the verification in the first reply including the comparison of the simulation results with photographical evidence and the hydrographs.

We incorporated levees and dams into simulations by changing both the surface DEMs and bedDEMs of dams and levees described in Fig.2.

Admittedly, there are many assumptions in our simulation work and we don't consider the changes of particle size before and after the dams and levees. The study of increase erosion around the sides of the dams and levees was been limited in C-L models owing to the unattackable settings near the engineering facilities. Actually, we pay more attention to the short-medium effectiveness of the interventions, which is present in regional features analyzed from the erosion and deposition in the total catchment and the output in the outlet. If possible, we would study from the smaller-scale by using other simulation models or field surveys in the future work.

The authors have demonstrated **a poor use of figures and tables** to support their results. Specifically,

> figures variously lack scales or annotations to indicate where the areas of erosion or deposition are (eg figure 5), or where other features (such as dams) referenced in the text are located (eg figure 1).
>
> Thanks for the suggestions. We added the scales in Fig5. in the first time, but they looked odd because the figure is just to show the distribution of erosion and deposition. At last, we decided to remove the scales and other annotations in the newest one.
>
> Some figure captions do not make sense eg figure 2 does not clearly show any chart or process for generating the bedrock DEMs. Some figures do not appear to contain the information described in the text.
>
> Thanks for the suggestions on figures and tables. We would check them carefully and revise them.
>
> Tables are present in the manuscript which are not referenced in the text; different tables share the same number (eg there are 2 tables labelled as table 2); and some tables do not identify what the units in the table represent.
>
> Admittedly, we made a mistake in table labels, we would check again and revised in the manuscript.

Finally, the author's use of **referencing is poor and inconsistent**.

Thanks for the suggestions on references. We have added some new research listed in each reply. We will check again and revise using the standard styles.

References

Li, C., Wang, M., Liu, K., & Coulthard, T. J. (2020). Landscape evolution of the Wenchuan earthquake-stricken area in response to future climate change. *Journal of Hydrology*, *590*(June), 125244. https://doi.org/10.1016/j.jhydrol.2020.125244

Skinner, C. J., Coulthard, T. J., Schwanghart, W., Van De Wiel, M. J., & Hancock, G. (2018). Global sensitivity analysis of parameter uncertainty in landscape evolution models. *Geoscientific Model Development*, *11*(12), 4873–4888. https://doi.org/10.5194/gmd-11-4873-2018

Wilcock, P. R., Asce, M., & Crowe, J. C. (2003). *Surface-based Transport Model for Mixed-Size Sediment Surface-based Transport Model for Mixed-Size Sediment*. *9429*(February). https://doi.org/10.1061/(ASCE)0733-9429(2003)129

Xie, J., Coulthard, T. J., & McLelland, S. J. (2022). Modelling the impact of seismic triggered landslide location on basin sediment yield, dynamics and connectivity. *Geomorphology*, *398*, 108029. https://doi.org/10.1016/j.geomorph.2021.108029

Xie, J., Wang, M., Liu, K., & Coulthard, T. J. (2018). Modeling sediment movement and channel response to rainfall variability after a major earthquake. *Geomorphology*, *320*, 18–32. https://doi.org/10.1016/j.geomorph.2018.07.022