

Comments by the editor

This manuscript has now received two detailed reviews, one anonymous, and one by J. Baronas (very detailed). Both these reviews provide a positive evaluation of the manuscript. Overall, I agree with the reviewers that this is a well written manuscript and well constructed piece of science and I encourage you to submit a response to the reviewers and a revised manuscript.

Thank you very much for facilitating the review of the manuscript. We now have gone through all of the reviewers' comments and replied to them point-by-point. Note that the comments are formulated in a way that reflects our already implemented changes in the manuscript.

In addition to the changes explained in the point-by-point replies, we re-ran the inversion with a slight change in endmember values that is due to taking the mean rather than the median of the rock endmember values as well as correcting a mistake in the silicate-endmember estimate for the flysch samples. This change modified the detailed position of the datapoints but did not alter the overall patterns described in the manuscript.

Although the comments by the reviewers are relatively straightforward to address, they are important and will help get this manuscript the visibility that it deserves.

All the comments of the reviewers be considered as a point by point basis and fully addressed in the revised manuscript. Based on my reading of the manuscript, and the reviewers, I suggest the following points be given careful consideration.

Missing references are highlighted by both reviewers. I think their suggestions are valid, and if possible, should be incorporated.

We incorporated most suggested references as explained in our responses.

R2 suggests that a more specific title might be useful, potentially providing a greater impact for this work and I think this is worth considering very seriously.

We modified the title to read: "The effect of lithology on the relationship between denudation rate and chemical weathering pathways. Evidence from the eastern Tibetan Plateau."

The source of sulfate raised by R2 is important. As a minimum I suggest the uncertainty associated with the assumption that all SO₄ is from pyrite be taken into account. What about hot springs for example?

Following the comment by R2, we removed the note on the potential differences between metamorphic and non-metamorphic sediments as explained in the detailed responses.

The points raised by R1 about the inclusion of rain and hot springs in the inversion model but then were corrected for confused me also. This needs clarification.

We modified and expanded the methods which hopefully clarifies all of the concerns and confusions that were caused by this section.