Referee #2

This letter presents a regression model for denudation rates based on publicly available data derived from cosmogenic nuclides. In a nutshell, my impression is that trying to compress it into a letter destroys a nice piece of work. I read the manuscript several times, permanently switching between the main text, the appendix, the supplementary text and figures and the supplementary tables. Finally, I think I understood the main ideas, but many questions remain open. To be honest, having agreed to review the manuscript was my main motivation not to give up earlier.
Written in a clear and reproducible way, this study might become a very good research paper. Since this would require a complete rewriting, I focus on a few points in the following and do not go deeper into details.

Reply: We are pleased that you find our study a nice piece of work. Thank you for your general and specific comments. They have enabled us to improve the text considerably and are addressed below.

We understand the point that summarising the manuscript in a letter format can make it difficult to read, as you have to move back and forth between the main text and the supplementary figures, tables and text. However, our strategy was to produce a brief text that would appeal to a broad audience, but which would also contain all the necessary material for those who wanted to read more deeply. We know that this strategy can make reading more difficult for people who want to analyse the text in depth, but we think the balance is still positive. In this new version, we have moved some of the supplementary material into the main text in order to provide a more agile and comfortable reading experience.

(1) The results section starts with some kind of promise that the model proposed here "would be the best physically plausible denudation prediction for a planetary scope so far." While some references are given, I did not find any serious discussion about this aspect (number of adjustable parameters, removing basins, ...). And given that the relation provided here is really the best one, what would we do with it?

Reply: We have now added the Table S3, which compares the metrics of our model with those of the other global denudation models (number of adjustable parameters, predictive capacity, etc.).

Having generated a good model, we were able to give our opinion on various uncertainties in the scientific community, such as the effect of bioclimatic condition on denudation, or how the effects of bioclimatic condition and terrain slope interact. These issues were analysed in the Discussion.

Also, as is mentioned in the last paragraph of the manuscript, the model can be useful to estimate the denuded mass that is exported to the sea, to improve the knowledge of the carbon cycle and to understand the limitations of the current landscape evolution models, where, for example, the water effect is generally reduced to the streamflow that is estimated through the upstream area, thus assuming that precipitation does not vary spatially.

(2) The parameters introduced in Eqs. (1) and (2) are not explained at their first occurrence and are partly not defined completely later. So it is practically impossible to recognize which of the covariates used here have the strongest effect on denudation. It is also not clear why the peak ground acceleration is taken into account in a different way than the other covariates.

Reply: This comment was also made by the other reviewer. We now clarify what the variables introduced in Eqs. (1) and (2) are and how they have been calculated.

A simple way to compare the effect of each covariate is to estimate the ΔY of each panel in the Figure S1a-e. It can be seen that slope has the larger effect, seismicity, bioclimatic state and cryospheric development have similar effects, and the abundance of hard lithology has the smaller effect.

Peak ground acceleration (PGA) is taken into account in a different way because, unlike the other covariates, it had a higher asymmetry and so its logarithm was used (Appendix C). By solving the logarithm of the dependent variable (denudation), the logarithm of PGA is expressed as indicated in Equation (1).

(3) The discussion section mainly addresses the limitations. After reading it, I was left with the impression that those aspect that could really deepen our understanding cannot be addressed.

(a) In the first section, it is admitted that the correlation of vegetation with other properties does not allow for a separation of its effect. In turn, it is stated in lines 122-123 that "positive sign of vegetation's factor loading in PC1clim suggests that vegetation resulting influence is positive for the analysed temporal windows." I do not understand why this is the case. To my knowledge, the loads only refer to the variability in the climatic components and not to their relation to denudation rates.

Reply: In fact, both sentences seem to be contradictory. We remove the sentence "positive sign of vegetation's factor loading in PC1clim suggests that vegetation resulting influence is positive for the analysed temporal windows" because we are not convinced that this is true. Regarding the second comment, the loadings of a principal component analysis indicate the coefficient of correlation between the original variables and the principal components.

(b) The second section discussed the contributions of rivers and hillslopes. In lines 145-146, it is stated that "The higher predictivity of variables related to hillslope suggests that for most basins the majority of denuded mass comes from there." I would agree that most of the denuded mass comes from the hillslopes, just owing to their larger area. Concerning the predictivity, however, it could also be that the properties used for characterizing rivers are more uncertain than those for slopes.

Reply: We agree that data quality could have affected the predictive power of the different covariates, in fact we try to reduce this bias by performing a test where all topographic covariates had to have the same spatial resolution (Appendix B). However, the preference for hillslope-related variables over those related to the drainage network is so significant that we believe this is partly due to more sediment coming from there.

(c) It was very difficult for me to follow the rest of this section (about the slope) and I am not sure what to learn from it.

Reply: We now explain this section in more detail and add key points. In summary, we found that water availability increases basin slope, and given that slope increases denudation, this is an indirect effect of water availability on denudation.

(4) Finally, I would suggest to compare the relate to those obtained by Harel et al. (2016, doi 10.1016/j.geomorph.2016.05.035). As far as I can see, these authors used basically the same data on denudation rates. As a major difference, these authors already tried to predict the parameters of the stream-power incision model for the rivers draining the respective basins. In its spirit, however, it is very similar, although the recent study is obviously more comprehensive concerning the covariates.

Reply: Thank you for the suggested paper, we have read it and compared its results with our results in the Table S3.

Overall, I think this nice work will be lost when published in its present form as a letter. So I would suggest to rewrite it and submit is as a research paper.

Reply: We have already responded to this comment.