Response to Reviewer #1:

[Comment 1] I enjoyed reading “A Revised Model of Global Silicate Weathering Considering the Influence of Vegetation Cover on Erosion Rate” by Zuo and his colleagues. In this paper, the authors effectively tackle the issues of overestimating the tropic weathering rate in existing weathering models, by incorporating the overlooked shielding effects of vegetation cover. This revision yields a more reliable global weathering flux, aligning closely with the modern observed value and degassing rate. The results are compelling, highlighting the importance of revising silicate weathering models, and thus they deserve to be published.

Response: Many thanks to the reviewer for recognizing the value of our study and providing valuable and insightful comments and advices, according to which we have made detailed revision to the manuscript.

[Comment 2] This study hints at a potentially diminished impact of land plants on the silicate weathering rate, in contrast to the enhanced effects of land plants via root wedging, or releasing organic acid. As shown in the paper of Mills et al. (2011) and other related papers, the availability of fresh rock can limit silicate weathering rates. The substantial soil formation likely resulting from plant growth could exert such an effect. However, the substantial formation of soil generally also requires a transport-limited regime, promoting soil preservation and hindering the exposure of fresh rocks. Consequently, I am curious whether the shielding effect of the leached soils is a result of vegetation development, relatively flat terrain, or a combination of both. Although the authors have tested two slope datasets in their study, a detailed comparison between the global soil distribution and slope grids is expected to address this concern. Inspired by this concern, I believe the initial phase of vegetation growth still plays an important role in the enhancement of silicate weathering, but a long existence of vegetation covers without considering the control of topography may exert a reduced effect on silicate weathering as this study shows. A short discussion on this is expected to be added to the implications for deep-time modelling.

Mills B, Watson AJ, Goldblatt C, Boyle R, Lenton TM. Timing of Neoproterozoic glaciations linked to transport-limited global weathering. Nature Geoscience 2011;4:861–4. https://doi.org/10.1038/ngeo1305.

Response: Thanks for your suggestion. First of all, we want to emphasize that the shielding effect is more likely due to vegetation not leached soils; the latter is mainly a result of the shielding effect of the former. Then, in order to answer the reviewer's question, two panels showing the slope and erosion rates calculated for two sets of surface slopes are added to Fig. S4 now. It can be seen that the regions with the lowest erosion rates are the desert regions where runoff is too small. Some low-latitude regions with relatively low erosion rates are consistent with the relatively small surface slope (i.e., flat terrain). However, in many places where the leached soil exists (Fig. S6b), the erosion rates are relatively high due to high runoff. These regions correspond more closely with the regions with high leaf area index (LAI). Therefore, leached soils should be a result of a combination of vegetation development and relatively flat terrain, but the flat terrain is not a necessary condition. A new section 4.3 has been added to the revised manuscript to show the discussion above.

We totally agree with the reviewer that the influence of vegetation on silicate weathering is two fold: it should enhance silicate weathering when there are not much vegetation, but may start to hinder the weathering through reducing erosion when the plants flourish, especially those with dense leaves and extensive roots. This point was made in section 4.1 of the original manuscript but is now emphasized again in the summary section

[Comment 3] As a whole, this paper makes a substantial contribution to the refinement of existing silicate weathering models. Aside from the minor concerns raised above, I have a few suggestions for improving the writing, which I provide in line-by-line comments.

Response: The encouragement and suggestions are greatly appreciated.

Comments tied to specific portions of the text:

[Comment 4] Line 37: (2023)? A reference is missing here.

Response: Thanks. This and all other reference-associated errors are corrected in the revised manuscript.

[Comment 5] Line 49: “If we want to”. I suggest avoiding verbal expressions like this, by using formal phrases of words. Please check it over the entire manuscript.

Response: Thanks for the suggestion, this sentence is rewritten. We also checked the entire manuscript to remove informal expressions as much as possible.

[Comment 6] Line 90: You have already cited this paper in the subject of this sentence.

Response: Fixed, thanks.

[Comment 7] Line 100: The citation style in sentences should be checked over the entire manuscript. For example, here the text “by (Gerlach, 2011)” should be changed to “by Gerlach (2011)” to be consistent with the context.

Response: All are fixed, thanks.

[Comment 8] Line 104: “Park 20” or “Park20”? Is there a space between? Please check it throughout the manuscript.

Response: It should be Park20. All occurrences of "Park 20" have been fixed.

[Comment 9] Line 107: “Os/Os”?

Response: Thanks for pointing it out, the correct form should be $^{187}Os/^{188}Os$ and is fixed in the revised manuscript.

[Comment 10] Line 125: According to the cited reference, it should be “as early as 1981”.

Response: The reviewer is right. The error is fixed.

[Comment 11] Line 132: “A simple solution” might be better than “a simple fix”. Try to avoid colloquial expressions.

Response: Thanks for the suggestion.

[Comment 12] Line 237: The parameter of “Xm” in Table S1 seems lacking in the equations in the main texts.

Response: "X_m" in Table S1 should be $(x|_{z=0})$, which represents the concentration of relevant cations in the fresh rock and appears in Eq. (9) of the main text. We apologize for the mistake.

[Comment 13] Line 248: what does the “2 m” mean here? Also, the meaning of “re-analysis” in this sentence is blurred.

Response: "2 m temperature" means air temperature at 2 meters above ground. It is written now as "2-meter air temperature" to remove any possible ambiguity. For "re-analysis", an explanation is now provided and the sentence is rewritten as "... obtained from ERA5(Muñoz Sabater, 2019). ERA5 is a re-analysis dataset obtained using a global climate model constrained by various observations from weather stations, ships, and satellites etc. The dataset is grided with a spatial resolution of 0.1°×0.1°."

[Comment 14] Line 323: Please check the reference style here.

Response: The sentence has been rewritten from "Due to this significant mismatch between the datasets, we used both the Gaillardet data and Gaillardet+HYBAM data to validate the model." to " Due to this significant mismatch between the datasets, we used both the Gaillardet et al. (1999) data (denoted as Gaillardet) and the combination of Gaillardet and HYBAM data (denoted as Gaillardet+HYBAM) to validate the model."

[Comment 15] Line 329: Please check the references here.

Response: Thanks, corrected.

[Comment 16] Line 428: “large” – “overestimated”.

Response: Thanks, the word "overestimated" is adopted.

[Comment 17] Line 538: "Equ" or "Eq."? "[13]" or "(13)"? the bracket style and abbreviations should also be consistent.

Response: Thanks, it should be Eq. (13). We have checked and corrected all such errors throughout the manuscript.

[Comment 18] Line 572: why 20% is chosen here?

Response: The threshold value 20% was chosen quite arbitrarily based on the distribution of leached soils. As shown in Fig. S6b, the areal fraction of leached soils within each grid cell is almost either >0.8 or none, especially in the low-latitude regions. Therefore, choosing a threshold value of 20% or 60% does not have much influence on the result. Such description is now added in the revised manuscript.

[Comment 19] Line 739: Eq. (15) and (15)? It should be (15) and (16), I guess.

Response: Thanks, it should be Eq. (15) and (16). In the revised manuscript, we have removed the original Eq. (15) and moved Eq. (16) to Eq. (15).

[Comment 20] Line 782: "than before" – "than previously thought in the models".

Response: Thanks, corrected.

[Comment 21] Line 820: Check the reference title here. In addition, the capitalization rules for the titles of the references should be consistent, in some references, the first letter of the first word is capitalized, but in others, the first letters of all the words in the titles are capitalized.

Response: Yes, the capitalization appears mixed largely because the title capitalization rule is different in different journals where the references were published. It is probably better to wait for the type editor to tell us whether we should unify the format.

[Comment 22] Line 1086: check the subscript here.

Thanks for the careful reading, the error is fixed.

[Comment 23] SI: the citation style in sentences should also be checked and improved.

Response: The citation in the SI has been checked thoroughly and the style is made consistent with that in the main text.

[Comment 24] Caption of Figure S7: it is unusual to directly include the panel letter in a complete sentence.

Response: All figure captions are modified so that they start with a summary sentence followed by description of each panel.

Response to Reviewer #2:

[Comment 1] The manuscript submitted for Geoscientific Model Development by Zuo, Liu, and other co-authors focuses on investigating what causes the discrepancies between the modeled and observed weathering fluxes in current 2D chemical weathering models. Such investigation is crucial for developing weathering models because the present-day condition is the most important criterion to evaluate models. Moreover, to know better the effect of each factor in models can bring some new thinking about the physical Earth.

The author conducted experiments that controlled factors of temperature, runoff, and slope from different data sources, and tested the effects of seasonal temperature variations, leached soil, and erosion related to vegetation cover. The experiments show the much lower erosion rates caused by vegetation in low latitudes are the most likely reason for the discrepancies because the difference becomes the smallest when vegetation effects are corrected.

The author presented a clear flow of investigation, first by the choice of datasets, followed by choosing a better statistical method and then investigating each factor through the same statistical method introduced before. With the revised model, the authors suggest a smaller sensitivity of silicate weathering to climate change. The experiments are of high quality, the results are convincing and the deduction is interesting.

Considering the theme targeted, the methodology used, and the conclusions obtained, the publication of this manuscript in a journal like Geoscientific Model Development is therefore justified. I have no major criticism for the manuscript and just several comments on the writing and figure presentation which are listed below. Other small mistakes and comments are marked in the document of the manuscript. The overall evaluation is a minor revision.

Response: Many thanks to the reviewer for recognizing the scientific significance of our study. We also benefit greatly from the valuable and insightful comments and advices given by the reviewer.

Major comments:

[Comment 2] The introduction of the paper does not fully and clearly present the motivation for this study which is embedded in main text. I suggest reorganizing the writing of the introduction and focusing on the improvements the authors want to make to make this section more concise. What the paper's motivation is for figuring out the overestimation of tropical weathering flux, it is better to structure the Introduction only following this topic.

Response: Thanks for the suggestion, the original Introduction was indeed slightly twisty. We have removed much information in order to get right to the point, while in the meantime, still describe the background thoroughly.

[Comment 3] The authors need to make it clear for the readers when they talk about the CO_2 consumption flux linked with Ca-Mg silicate weathering and total silicate weathering (Na- and K-silicates do consume CO_2 at modern scales, but consume less on geological scales through cation exchange, e.g., France-Lanord and Derry, 1997 https://doi.org/10.1038/36324), as well as the long-term CO_2 consumption from silicate weathering (considering CO_2 release when carbonate has precipitated, and less efficient CO_2 consumption by Na-K silicate weathering) and silicate weathering on modern settings (all weathering flux of Na, K, Ca, Mg from silicates). For example, in L102, the global total silicate weathering flux Fw is only 2.5 Tmol/yr, which is exactly the Ca-Mg silicate weathering flux.

Response: Thanks for the suggestion, we should have made it clear for general readers that only Ca-Mg related silicate weathering is focused upon in our study. The following description is now added in the subsection 2.1.a:

"It should be noted that the global total silicate weathering flux throughout this work pertains specifically to the weathering flux of Ca- and Mg- silicates. While Na- and K- silicates also participate in weathering, these are not traditionally regarded as carbon sinks on geological timescales (Berner et al., 1983) due to their inability to form carbonate minerals. However, the residence times of Na+ and K+ in the ocean are ~80 Myr and ~10 Myr(Lécuyer, 2016; Emerson and Hedges, 2008; Olson et al., 2022; Berner and Berner, 2012; Hu et al., 2020), respectively. This long residence time means that the weathering of Na- and K- silicates do have an impact on the atmospheric CO_2 on million-year timescale.. Moreover, Na^+ and K^+, when released into the soil through weathering reactions, may displace Ca^{2+} and Mg^{2+} in rocks through cation exchange with sediments or oceanic crust (France-Lanord and Derry, 1997), leading to carbonate deposition and carbon sinking indirectly. However, currently we are unable to quantify these aspects due to the intricacies of the Na and K cycles. Thus, we focus solely on the Ca^{2+} and Mg^{2+} silicate weathering flux in the current study."

[Comment 4] Goddéris developed improved GEOCLIM models that incorporate the shielding effects of thick regolith on chemical weathering (Goddéris et al., 2008 (10.1016/j.geoderma.2008.01.020); Goddéris et al., 2017 (10.1038/ngeo2931)), which aims at normally the same goal as this model study. I think it will be better to include the comparison with his studies in the discussion. The interesting point for this comparison is that the authors' results ascribe the low weathering flux in low latitudes to the decreased erosion, and ultimately to vegetation, while their works are based on the idea that the thick regolith prevents the ongoing weathering process and fluid is hard to reach the fresh bedrocks. I suggest discussing the two kinds of understanding for the low weathering flux in low latitudes.

Response: Thanks for the suggestion because we had only cited the work of Hartmann et al. (2014) in the original manuscript when discussing the shielding effect of thick soil. Godderis et al. (2008) considered the effect of thick regolith cover on weathering by reducing the fluid that can reach the fresh bedrock, while we consider the effect by reducing erosion rate. The two approaches agree with each other in that they both think that the weathering is transport limited (fresh rocks are not exposed for weathering), but our approach is more direct and easier to be applied to the paleo periods. This is because 1) the existence of the thick regolith cover is likely the result of weakened erosion, and 2) the knowledge of regolith cover and thickness is unavailable for the past. Therefore, it seems better just optimize the parameterization for the erosion rate directly, such as by considering the effect of land plants. Such discussion has been added to the section 4.3 of the revised manuscript.

[Comment 5] L38 citation missed

Response: Thanks, fixed.

[Comment 6] L42 How about the oxidation of the sulfide as a carbon source

Response: Yes, this is definitely a possible source of CO_2 and was neglected in the original manuscript. We have removed all description about the source of CO_2 in the revised manuscript in order to shorten the Introduction as suggested by the reviewer.

[Comment 7] L50-52 Here the authors mentioned numerical calculation, which, however, brings some questions why do the authors choose the numerical calculation to know the present-day silicate weathering? Why don't choose to improve the field observations on the basis of Gaillardet et al. (1999)? I suggest structuring the introduction more clearly, explaining the shortcomings of other methods first and then moving to the topic of numerical calculations and showing its advantages, e.g. reconstructing past weathering fluxes.

Response: Thanks for this useful suggestion. A new paragraph is added in the Introduction:

"One of the essential ways of determining the carbon sink is through numerical modeling, especially for that in the deep past. Numerical models not only provide the magnitude of carbon sink, but also allow us to study its sensitivity to various factors such as continental evolution and climate change. Our goal here is to improve the model calculation of the primary sink of CO2, that is, the silicate weathering, with a focus on its present-day values for which the spatial distribution is relatively well constructed."

[Comment 8] L106 The superscripts of the Os isotopes are missed.

Response: Thanks, the correct form $^{187}Os/^{188}Os$ is used in the revised manuscript.

[Comment 9] L153 I think tectonic movement is not the only way that let the unweathered rock move upward. The climate-induced isostatic rebound can also be a factor.

Response: Thanks for pointing this out. However, we do not think that this process is very important on the timescale concerned herein. The postglacial rebound reaches near its final state (i.e., the ground stops rising) on a timescale of 10,000 years ((Peltier, 1974); Fig. 8a of (Liu and Richard Peltier, 2013)) unless there is continuous loss of glaciers on longer timescales, which is unlikely.

[Comment 10] L155 The authors didn't mark the height of the soil in Stage 2 but in Stage 3.

Response: Yes, the soil layer is marked in Stage 3 only for the sake of visual clarity. In the revised manuscript, we now provide an explanation in the caption of Fig. 2.

[Comment 11] L179 Steady state is a basic assumption in the model. I think it is necessary to demonstrate to the readers that this assumption is feasible for the main question of this paper.

Response: Yes, the steady state assumption is indeed a critical assumption of the weathering model employed herein, its validity should be demonstrated. The weathering profile may be considered to have reached a steady state on the timescale of million years if its lifetime is much shorter than this timescale. The lifetime of a weathering profile may be estimated by using its typical thickness and the surface erosion rate. From Fig. 3 of the main text, it can be seen that a lower-bound value for erosion is 60 ton/km^2/yr (equivalent to 2×10^{-5} m/yr). Then, for a typical thickness of 10 m, the lifetime is half a million years which will be much shorter for regions with higher erosion rate. Therefore, a weathering profile is near steady state if the environment changes slowly over a few million years. Such an assumption is not ideal but is necessary to make in order to study the long-term (hundreds of millions of years) evolution of silicate weathering. Such description is now added in the revised manuscript near lines 184-194.

[Comment 12] L200 It is better to briefly introduce what Park's parameterizations mean for d0 and k1.

Response: We apologize for the unclear description here. k_1 is set to zero in Park's parameterization, meaning that the soil production rate decreases monotonically with the thickness of the soil. d_0 is set to 2.73 m. The description here has been substantially rewritten in the revised manuscript.

[Comment 13] L248 What does this "2 m" mean?

Response: Sorry for the ambiguous description here. "2-meter air temperature" is now used, hopefully to have removed the ambiguity.

[Comment 14] L329 Citation missed?

Response: Thanks, fixed.

[Comment 15] L407 This is the first place where "R2" appears, but it has not been defined yet.

Response: Thanks, "R2" is now defined here rather than later in the revised manuscript.

[Comment 16] L435 For Figure 4, the presentation for different temperature datasets with different colors is not good because too many colors and symbols are hard to read and the authors didn't put a legend beside them. First, the authors want to separate the ERA dataset into three time periods to show if global warming has effects, but colors cannot show the time sequence clearly. Moreover, the green symbols are hard to see in this figure. I suggest changing the way of presenting this figure.

Response: Thanks for your suggestion. In the revised Figure 4, legends are added below to show what each symbol stands for. Moreover, only the results for one time period of ERA temperature are shown in (a) and (b) since the three periods give very similar results. In this way, the red, green, and orange colors are reduced to only one color.

[Comment 17] L514 The sample dot in the legend is too small to read its color.

Response: Thanks, we have increased the size of the sample dot substantially in the legend of Fig. 6 in the revised manuscript.

Response to Reviewer #3：

[Comment 1] This manuscript is a valuable revisiting of an existing weathering model (Gabet & Mudd 2009, West 2012, Maffre et al. 2018 and Park et al. 2020). The authors identified a systematic bias in the parametrization of the model, and pointed toward a potential explanation. Although they didn't rigorously prove that they identified a missing process (reduction of erosion rates by vegetation), they provide useful hints. They conducted a thorough analysis of the sensitivity of the model, and its optimization method, that is well documented in the manuscript. The text is well written (considering that I am not a native english speaker) and well organized.

Response: Many thanks to the reviewer for your thorough review of our manuscript and instructive comments and advices. We have tried our best to address each point you've raised below and all of them are also reflected in the revised manuscript.

[Comment 2] The only main comment I have is that the author give the impression that they tried and tweak the erosion with ad hoc modifications so as to reduce tropical erosion, with the only purpose of improving the fit to the weathering data. If the overestimation of tropical erosion is the cause of the overestimation of tropical weathering, then improving the fit to the erosion data should be a prerequisite to improving the weathering fit, not something to check afterward. The danger of such modification is to generate a better weathering model for the wrong reason. For instance, is the drastic erosion reduction by an order of magnitude where leached soil is observed (line 574) consistent with erosion measurements? The authors did make that verification (Fig. 10 a and b), but did not put it forward enough. It should appear at the beginning of section 3.4, not merely cited in the middle of section 3.5 (line 620-621). Moreover, they only made this verification for one of their erosion modification (reduction according to global LAI), and provide no information about the other erosion modifications.

Response: We agree with the reviewer that our purpose or motivation is to improve the fit of modeled weathering fluxes to the observations. We first show that the silicate weathering over the tropical region was systematically overestimate, which leads to an overestimation of the global weathering flux. Then, by exploring multiple possibilities, we found that the previous overestimation was likely due to the overestimation of erosion within the tropical region. The reason that we suspect that the erosion was overestimated by models is primarily based on the comparison of model erosion to the cosmogenic nuclide analysis data (Fig. 3b) and the extensive leached soils within the low-latitude region. Moreover, reducing erosion at where leached soils are present does improve the fit to both the observed erosion rate and the weathering fluxes, supporting our suspect. Therefore, we were not trying to make ad hoc modifications to the erosion but trying to demonstrate step by step that reducing the tropical erosion rate was the most reasonable solution. We do apologize for giving the reviewer such an impression, most probably due to the way the results were presented as pointed out in detail by the reviewer.

The reviewer's concern on whether we only care about the effect on weathering while ignore whether the modification in erosion is reasonable is very understandable. We did check the effect on erosion to make sure the reduction of erosion where leached soil is observed is consistent with erosion measurements. Moreover, as the reviewer knows well, the tunning of the parameters (Table 2) of the weathering model has no influence on the calculated erosion rates (Eq. (7)). That is, if the erosion rates are not improved in the first place, the weathering calculation would not be improved by parameter optimization, as has been demonstrated in section 3.2.

We added Fig. 9 shows the effect on erosion rates by various methods mentioned in the manuscript. As can be seen, the erosion rates are improved in all cases. We also understand that although the proposed method of improving the erosion calculation is practical and seemingly plausible, it is quite crude. A lot more work is clearly needed in the future from both the observational and modeling sides to improve the erosion calculation comprehensively.

[Comment 3] Perhaps even more critical is section 4.2, where the indicated aim really seems to be the improvement of the computation of erosion (influence of runoff), but is achieved by optimizing the weathering fit.

Response: Many thanks for pointing out this problem in structuring and writing. Section 4.2 is modified now according to the spirit of the reviewer's comment; the effect on erosion by removing the influence of runoff is demonstrated first.

[Comment 5] Another interesting point that should be more highlighted (in the discussion and summary, for instance) is that the improvement of weathering computation rely on trusting the cosmogenic nuclide-derived erosion rates rather than the TSS-derived ones. I understand, however, that given the limited amount of basins with cosmogenic data in the current dataset, it may not be useful to optimize the weathering parameters with cosmogenic erosion correction ("_Be" cases).

Response: Yes, we agree with the reviewer that there is this limitation on the observational side and should be pointed out more clearly. We think that although the Total Suspended Solids (TSS) contains information for many more rivers but is subject to uncertainties that the suspended solids may not represent the erosion over the river basin; for example, the suspended solids could have been eroded primarily from near the river mouth, overestimating the erosion, or substantial deposition has occurred on the way, underestimating the erosion (Wittmann et al., 2020). Moreover, the TSS data may very well be influenced by human activities (Hewawasam et al., 2003). The erosion from the cosmogenic nuclides analysis, on the other hand, may be more representative of the long-term erosion, but is available for not so many rivers. A paragraph is added in the section 4.5 of the revised manuscript to fully describe the limitation with the erosion data.

With the current work, our goal is to bring up the overestimation problem and come up with a solution, hoping to stimulate the community to find other and maybe better solutions.

Other comments:

[Comment 6] The authors should indicate more systematically what model cases and forcing fields (temperature, runoff, slope) where used when they present the results (Fig. 6, Fig. 8, Fig. 9 …) see my specific comments after.

Response: Thanks for pointing this out. The case names are now provided in all the relevant figure captions in the revised manuscript, the forcing fields are also described when necessary.

[Comment 7] In particular, why the modeled erosion in Fig. 10a looks so different than from Fig. 3a? Is it because different forcing fields where used?

Response: We apologize for the confusion caused by not clearly describing the experimental data for each figure. Fig. 10a displays erosion rates from cosmogenic nuclide analysis and should thus be compared with Fig. 3b. Moreover, Fig. 3 utilizes slope $s1$, whereas Fig. 10 uses slope $s2$; different slope data induces some minor difference between Fig. 10a and Fig. 3b. However, we have moved the Fig.10a, b into Fig. 9, and we have make clear explanation of the forcing fields, and also the Fig. 10 has been changed to Fig. 11.

[Comment 8] It is not completely clear how the authors performed the month-by-month weathering computation. What I understood (lines 397-399) is that they used the monthly climate fields and run the full weathering model (computation of erosion, regolith thickness and weathering) with them. This method is not realistic because it computes a steady-state for each month. Yet, regolith thickness would not vary seasonally: a 10cm deep regolith with an erosion rate of 1 mm/yr has a response time of 100 year, a 10m-deep one with an erosion rate of 0.01 mm/yr has a response time of 1 million years. Similarly for the vertical profile of x (eq. 3). To properly evaluate the effect of the seasonal cycle, the full dynamic equations of the model (Eq. 1 and 3) should be used. Or, as an approximation, the regolith thickness and x profiles should be computed with long-term annual mean climate fields, and used with the monthly climate fields to derive W (Eq. 9). But given that is not a key results, the authors can consider simply removing this analysis.

Response: Thanks a lot for the suggestion. We decide to take the suggestion and remove this entire subsection in the revised manuscript.

[Comment 9] It is surprising to see, in Fig. 4 (and lines 459-460), such a huge improvement of non-log r2 with the cosmogenic nuclide erosion correction ("_Be" case), given that this correction concerns only a limited number of basins (18). I have a feeling it is because of an "Amazon bias". Non-log r2 is heavily influenced by the high fluxes (since it is computed with absolute and not specific fluxes). The value of r2 is likely entirely determined by how well the few big rivers are represented (Irrawady, Ganges, Amazon…) Among those, The Amazon exhibits the biggest weathering mismatch (Fig. S7), and also the biggest difference between TSS-derived and cosmogenic nuclide-derived erosion rate (Irrawady and Ganges show smaller changes, Table S3). Some of the conclusions, like the preference for cosmogenic nuclide erosion, may rely, to a certain extent, to the data differences on the Amazon.

Response: Again, thanks for the very careful reading of our manuscript. The reviewer is right that non-log r^2 will be more easily influenced by rivers that have large absolute errors than the log r^2 measure. In particular, the improvement in the non-log r^2 is dominated by the improvement over Amazon region. Although log r^2 has the merit of giving more weight to rivers with small values and small absolute errors, it has the obvious disadvantage of misfitting the global sum; the absolute error of a few large rivers can shift the global sum significantly. Therefore, we think it is worth combining non-log r^2 and log r^2 to measure the model-data misfit.

[Comment 10] Lines 113-115: This affirmation should be a bit nuanced, given the results presented lines 707-714, showing than the relative sensitivity is not altered, though the absolute sensitivity is.

Response: Yes, the statement here should not have any preference how the climate sensitivity of silicate weathering should change with the model modifications. The sentence is modified to "the climate sensitivity of the silicate weathering, i.e., the ability of silicate weathering to stabilize climate, may be misestimated due to this systematic error". Moreover, the entailing sentence "This latter point will be demonstrated in section 4.1 near the end of this paper." is removed.

On the other hand, although the relative sensitivity turned out almost the same when the model is modified, we believe that changes in absolute sensitivity could still have an impact on our understanding of how the carbon cycle responds to climate change. For example, the relative importance of other carbon sinks (e.g. organic carbon burial) may change.

[Comment 11] Lines 527-528: Would seasonality still influence the soil production function at several meters depth, where the annual oscillations should be largely dampened?

Response: It is true that seasonal cycle does not penetrate deeper than a few meters, but we decide to keep this section based on the following considerations: 1) strong seasonal cycle helps shattering rocks when the regolith thickness is small, as in mountainous regions; 2) in mid- to high-latitude regions, strong seasonal cycle is able to produce deep cracks in the surface soil layer (Liu et al., 2020; Fortier and Allard, 2005), which may help the infiltration of fluids and thus the chemical weathering of rocks below the soil layer.

[Comment 12] Line 601: Why evergreen forests, and not all types of forest?

Response: This was done in the initial tests of the idea, but as is shown in later part of this section, there is no need to choose specifically the evergreen forests. Therefore, we have removed the description of such tests, as well as Eq. (15), in the revised manuscript.

Minor corrections:

[Comment 13] Line 17: "complicated" seems more appropriate than "complicate".

Response: Yes, thanks.

[Comment 14] Line 38: Something's wrong with the citation "(2023)". This occurs in a few places, like line 329: "(1991; ...)". Please check the citation tool.

Response: Thanks, all such errors have been fixed.

[Comment 15] Line 56: And CO_2. A direct dependence of silicate weathering on pCO_2 is often considered (like in Walker et al. 1981).

Response: Yes, indeed, we should have also included the description of this factor.

[Comment 16] Line 61: "A reason why we need" seems more appropriate than "A reason that we need", in that sentence.

Response: Agree, thanks.

[Comment 17] Lines 137-138: Shouldn't you say "it is found that"?

Response: Yes, a word "that" was missing and now added in the revised manuscript.

[Comment 18] Line 170: The power-law dependence is applied to τ, not to q.

Response: We are sorry for expressing it wrong, the runoff influences the weathering rate through an exponential form in Park20.

[Comment 19] Line 183 (Eq. 4): The substitution of τ by z/Pr holds under the steady-state assumption (mentioned line 179). It should then be helpful to specify dx/dt = 0 (= Pr dx/dz …)

Response Thanks for the suggestion, according to which Eq. (4) has been modified in the revised manuscript.

[Comment 20] Lines 195-201: The introduction of the 'humped' law with k1 set to 0 is a bit confusing. It would seem more clear to me just to talk about the exponential form. Or to mention the 'humped' law, while specifying that Park et al. (2020) used the exponential form, which is equivalent to the 'humped' function with k1 set to 0.

Response: Thanks for the suggestion, we have rewritten this part as:

"However, it has also been suggested that there is an optimum regolith thickness, soil production also slows down when the regolith is too thin under certain environments (Anderson, 2002; Strudley et al., 2006). The soil production rate has thus been described by the so-called 'humped' law,

$$P_r = k_{rp} \cdot q \cdot e^{-\frac{E_a}{R}\left(\frac{1}{T}-\frac{1}{T_0}\right)} \cdot (e^{-\frac{h}{d_0}} - k_1 \cdot e^{-\frac{h}{d_1}}) \quad (6)$$

where the second exponential term in the brackets is to ensure that the soil production rate decreases when *h* is too small. Here we neglect this effect by setting k_1 to 0, the same as in Park20. In Eq (6), k_{rp} is the regolith production constant to be determined by fitting the observations, d_0 is the attenuation depth and is set to 2.73 m, also the same as those in Park20."

[Comment 21] Lines 229-231: Again, it will be useful to remind here that Eq. 5 is obtained with the steady-state assumption E = Pr(h).

Response: Thanks, the steady-state assumption is mentioned again here in the revised manuscript.

[Comment 22] Line 254: “from Yves” is too uninformative. Is it from a personal communication with Yves Goddéris? Or is it just the name of the runoff field in Park et al. (2020) data repository?

Response: It is the name of the runoff field in Park20’s data repository. The sentence is revised to facilitate a better understanding:

“the other was from the runoff file marked by 'from Yves' in the data repository supplied along with Park20 ...”

[Comment 23] Line 261: Is “1950-1921” actually “1950-2021”?

Response: Thanks, fixed.

[Comment 24] Line 338: 18 rivers are indeed referenced in Table S3, but 19 rivers are mentioned after (line 349 and line 639).

Response: Thanks for pointing this out, it is supposed to be 18 rivers all through.

[Comment 25] Lines 341-342: Is B left as 1 for basin with no erosion data (for the correction with isotope-derived erosion rates)? This needs to be specified.

Response: When using cosmogenic nuclide analysis data for erosion rate correction, the corrections made with the TSS data are kept for where the cosmogenic nuclide data are unavailable. Only when neither TSS nor the cosmogenic nuclide data are available is *B* set to 1.

We apologize for not making this clear and have added the following description in the revised manuscript

"Moreover, if neither TSS nor the cosmogenic nuclide data is available for a river basin, *B* is set to 1 for this basin." at the end of subsection 2.2.e near lines 363.

and

"the erosion rates are calculated in three different ways which all used Eq. (7) but the parameter *B* has different values: *B*=1, *B* tunned according to TSS data, and *B* tunned according to both TSS data and the cosmogenic nuclide analysis. In the last case, the cosmogenic nuclide analysis supersedes TSS data if both of them are available for a basin." In the first paragraph of section 2.4 near lines 415-419.

[Comment 26] Figure 3: It is not indicated which slope and runoff field were used for the current model computation.

Response: We apologize for missing such information in many figures. For this one, we have added "… R_Yves and *s*1 (both defined in section 2.2) are used for the runoff and slope, respectively." in the figure caption.

[Comment 27] Lines 399-401: It could be specified that this specific experiment corresponds to the last line of Table S4.

Response: Based on one of your suggestions above, we agree that calculating monthly weathering fluxes is unreasonable, and thus the experiment should be removed. Consequently, this expression here is also eliminated in the revised manuscript.

[Comment 28] Lines 404-406: Perhaps indicate here that the erosion correction is not used (B factor left as 1) for this second set of experiments?

Response: Thanks, this is added in the revised manuscript.

[Comment 29] Line 417: It is said later (lines 610-611) that this is achieved by re-scaling the "ke" constant rather than "*B*", which seems more natural: ke is a global constant while *B* is meant to tune locally (basin by basin).

Response: Yes, it should be "k_e" here, we apologize for the mistake.

[Comment 30] Line 420: You should introduce here the experiment name "m5", from Table 1.

Response: Thanks, m5 experiment should be introduced here to improve the readability. In fact, in line with the response to the reviewer's comment 12 above, experiments m1-m3 have been removed and m4-m7 are moved forward accordingly.

[Comment 31] Lines 21 to 26 of Table 1: The experiment names (1st column) contain "*s*2" while the indicated slope field (4th column) is "*s*1".

Response: The slope field in the 4th column should be "*s*2" and is now fixed. Thanks.

[Comment 32] Line 495: Perhaps indicate that the blue dots are not showing on Fig. 6a-b precisely because R2 is <0?

Response: Thanks for the suggestion, this is now indicated in the revised manuscript.

[Comment 33] Table 2: The upper case "Σ" (3rd column) should be (I assume) the lower case parameter "σ".

Response: Yes, thanks.

[Comment 34] Line 538: Odd formatting of "Equ [13]".

Response: Fixed, thanks.

[Comment 35] Line 541: You could also refer to Fig. 8a, where the 12 combinations can be guessed.

Response: Thanks, Fig. 8a is now referred to here.

[Comment 36] Lines 543-545: Please indicate (here or on Fig. 8a) which curve of Fig. 8a corresponds to these best-fit "a, b and c" parameters. The color/linestyle code of Fig. 8a is no explained.

Response: Thanks for the suggestion. A legend is now added to Fig. 8a and further elaborated in the figure caption. Explanations for the two curves corresponding to the optimal parameters are also added near lines 560-563.

[Comment 37] Lines 548-549: this information ("only the envelope") should be on Fig. 8's caption. And what are the few dots that are still shown around the envelopes of the data points, on Fig. 8 and the following ones?

Response: We apologize for not providing a detailed explanation in the original version. The information about the envelope is added in the caption of Fig. 8 now. The few dots that are still shown are the data points used to obtain the envelope through curve fitting (cubic spline interpolation). This information is also included in the revised caption of Fig. 8.

[Comment 38] Fig. 8: The runoff field that is used here is indicated, but not the slope field. The temperature field could be indicated too, even though Table 1 indicates that it is always T_CRU for this series of experiment. Or simply the name of the experiment can be given.

Response: Thanks, the forcing fields are described in the figure caption, which is probably more convenient for the readers than only giving the experiment name.

[Comment 39] Lines 563-564: That is the case for tropical flat region, but isn't true for mountainous ones.

Response: Yes, this is now pointed out in the revised manuscript.

[Comment 40] Fig. 9: The temperature, runoff and slope fields that were used are not indicated, nor is the name of the experiment.

Response: We apologize again for missing the information, and all these are now added in the caption of Fig. 9 (which is Fig. 10 by now).

[Comment 41] Lines 604-621 Please remind here the "m1-m4" nomenclature, to better indicate which experiments those results correspond to, especially for when presenting the maximum R2.

Response: Thanks, both "*m*4" and "*m*5" are mentioned again here but they have been changed to "*m*1" and "*m*2", respectively, in the revised manuscript. Moreover, "*m*6" (changed to "*m*3") and "*m*7" (changed to "*m*4") are also mentioned in section 4.4.

[Comment 42] Lines 630-631: Do you mean the results were good with NCAR tropical LAI, but deteriorated with LPJ global LAI (m1-m2 versus m5)?

Response: Yes, the results with "*m*5" is worse than with "*m*4". This is understandable since the vegetation data from NCAR is and observational dataset.

[Comment 43] Fig. 10: Similarly, the input fields of temperature, runoff and slope are not indicated.

Response: Thanks, all information are now added to the caption of Fig. 10 (which is Fig. 11 by now).

[Comment 44] Line 780: Extra word in "model revised model".

Response: Thanks, the first "model" is now removed.

Supporting Information:

[Comment 45] Caption of Fig. S5: Wrong formatting of citation "(Hartmann and Moodsorf, 2012)".

Response: Thanks, fixed.

[Comment 46] Line 32: Which observation data, TSS or cosmogenic nuclide?

Response: TSS data is used here and is now described in the caption of Fig. S6. Thanks.

[Comment 47] Line 49: Which temperature, runoff and slope fields were use for the Park20 model results?

Response: All the forcing information as well as the experiment name are now added in the caption of Fig. S7.

[Comment 48] Line 56: There is no Fig. 8e

Response: It should be Fig. 11d and is now fixed, thanks.

[Comment 49] Table S3: The units of 10Be-derived erosion (6th column) is not indicated, though it seems to be the same as all others (t/km2/yr).

Response: Thanks, the units are added.

[Comment 50] Caption of Table S5: You should add here a reference to main text Eqs. 13 and 14.

Response: Thanks, the reference to Eqs. 13 and 14 of the main text is now added in the title of Table S5.

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