

## ***Interactive comment on “Water limitation may restrict the positive effect of higher temperatures on weathering rates in forest soils” by Salim Belyazid et al.***

**Salim Belyazid et al.**

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Received and published: 4 April 2019 The paper by Salim Belyazid and coauthors reports the results of modelling of the effects of predicted future climate change, with respects specially to changes in soil temperatures and soil moisture, on future chemical weathering rates in forest soils. They use an 'integrated forest ecosystem model' known as ForSAFE to model chemical weathering on 544 managed forest sites distributed through Sweden. They derive their future climate predictions from two general circulation models. From their modeling, they conclude that soil moisture will decrease

C1

in summer under predicted future warming, which could limit expected future increases in weathering rates attributable to higher temperatures. I write this review from the perspective of someone who is not a modeler but rather is a geomorphologist with knowledge of weathering. I thought the paper was really disappointing, given that this is an interesting topic and that the paper started reasonably well. The Abstract was clear and concise, the Introduction set the problem up, and the Methods were explained. However, from there the manuscript deteriorates with some fairly uninspiring results, a discussion that reads as esoteric and largely irrelevant to the results, and no Conclusions. The authors discuss various shortcomings with weathering models but offer no interpretation of their Results. There also seems to be almost no connection between the model and the physical environment, despite using 544 forest soil sites (boreal forest, soils developed on glacial diamicts, temperate to Arctic climatic range, mineral derived nutrients, podzols, etc, are all terms reflective of the physical environment that I was looking for but which were missing). Also, the important effects of soil erosion on chemical weathering rates are completely overlooked and there is no consideration of the temporal evolution of these soils derived from glacial sediments, which may be largely unrelated to climate. For example, what happens over time as easily available mineral nutrients in the sediments are depleted? Is there any bedrock weathering and how might that change in the future? The soils on which many of these forests are located are frequently thin and clastic. What about the effects of fire under a future warmer climate on these managed forests, in terms of nutrient uptake, nutrient cycling, effects on surface runoff, and vegetation assemblages, which might also be important enough to at least warrant a mention? The thread of their narrative is difficult to follow and I missed having a Conclusion section. The paper simply finishes with this sentence: “To reduce the uncertainties connected to those regressions, a revision of the regression is required, where the newest technology can be used”. It really says nothing, which in a way sums up this paper. It's a pity because I think the modelling exercise that the authors have completed has merit and it could be worthy of publication, but certainly is not, in my view, in its present form.

C2

Response to the reviewer's general comments: We appreciate all the aspects taken up, but we want to focus the paper on the effects of climate in the coming 80 years or so. Under this time period, we do not expect any significant physical changes, and want instead to focus on the interplay of moisture and temperature, to see if the 10% increase in weathering per 1 degree increase in temperature will materialise. However, we agree that the paper will benefit from a more thorough description of the results and more in depth discussion (this is in line with the message from reviewers 1 and 2).

15: Previous estimates of weathering rates? And what about temperature controls on reaction kinetics? 20: For those from a different field, it could be worthwhile explaining 'productive'. We can guess what it probably means but it would be good to have it defined. Also, what is modeled? 25: Elevated air temperatures should appear earlier 30: This could be shortened 35: It is unclear what exceedance of planetary boundaries means, its connection with climate change, and its relevance for this paper. 40: Why is an increase in biomass production needed to meet the stated goals? 50: Grammatical error with the references. Be specific and substitute tree harvesting for forestry! 55: Compensation potential – with respect to acidity? The reader can often guess what you mean but the writing should be much clearer. 60: Losses of what? 65: Accelerated is preferable to higher because the latter might mean larger trees. 70: Could be worth explaining radicals or being more specific with respect to listing those. Also the number and intensity of wetting and drying cycles might be important. "Net effect is positive or negative" be explicit: "on chemical weathering of soils". Also weathering encompasses physical and chemical processes, and physical processes related to freeze-thaw/frost cracking will change in a different way to chemical processes, under a warmer climate. 80: Are you explicitly modelling all of the unsaturated zone? What about in locations where it extends below the maximum rooting depth? What is the maximum rooting depth? 90: "and" organic cations. And "H<sub>2</sub>O" with the 2 as subscript. 95: What are the different layers of the simulated soil? 125: Hydrology-related fluxes of what? 130: No previous mention of the listed atmospheric pollutants. Explain their importance to this study and do this earlier in the paper. 150: Explain why you use these two particular

C3

models: ECHAM5 GCM and CCSM3 GCM. Is "story line" the correct term?

Thank you for the thorough comments. We will try to address them as we revise the text where we understand the request and where we see its relevance (we don't for example understand the question about planetary boundaries, productive forests or losses in line 60). Other instances, such as the request for clarification about if we model the unsaturated zone, will be addressed. Note to editors: we suspect that there is a mismatch in the line numbers referred to by Dr. Goodfellow and the version available from the journal. But we will try to look for the text indicated by the reviewer and not the line numbers.

RESULTS: In general, I found this section to be hard to follow. 175: Temporal or spatial 'variability'? 175-180: These two sentences read as being in conflict with each other. 190: Is it really both climate models? In the CCSM result in Figure 4 it appears that variance increases with soil temperature increase. 195: There really is no correlation between annual weathering rate and predicted moisture change. Is the correlation really significant? You are explaining 5% of the variance. The regression line very weakly indicates that annual weathering will increase with reduced soil moisture. Why would this be so and if you consider this result to be significant then why isn't it further explored in the Discussion? The text in line 175 refers to table 1, where the data is described as annual average for two periods. The variability is between the sites, so spatial. We will try to make this more explicit in the text. 175-180: sentence 1: there is a large variability in weathering among the sites, sentence 2: weathering will increase consistently at all sites due to climate change. The two sentences refer to different things (1- what is the weathering rate vs. 2- will it increase with climate change?). We struggle to see how these two are in conflict. 195: we agree, the correlation is insignificant, we do not understand this comment and do not disagree with the reviewer even if our formulation is not the same. Yet, as there is no correlation, why should we try to explain the very weak, insignificant trend? We do not think this is significant, as we have a statistical test telling us it is not.

C4

DISCUSSION: There is essentially no exploration of the results but lots of esoteric model details. The sentence below 230 is essentially a repeat of 230. Part of the disparity in what? 235: Geophysical properties – what about geochemical properties? Soil coarse material – this is the first mention of grain sizes and their importance to the study needs to be introduced near the start of the paper. “Fine earth” – is that silt + clay? “Estimation method” – what is being estimated? 240: “User defined possible materials” sounds like something out of a robotic University HR department. It is incomprehensible here. Also a concluding sentence is needed for this paragraph. What is the key point for the reader? 250: It is unclear what this sentence means 265: It is entirely unclear what the concluding sentence to this paragraph means. 275: Exposed mineral surface – is this reactive surface area? We will restructure and rewrite the discussion and conclusions, as also requested by other reviewers.

CONCLUSIONS: Where are they?

TABLES AND FIGURES Table 1: Might be better in a plot!? Define “future climate” in the footnotes. It is not a constant, but rather is evolving. 370: What is “increment of weathering”? 385: Define BC in the caption. 390: Figure 4: only the bottom left hand figure shows a correlation, and it is weak. 400: Figure 6: Is it even worth showing these? The highest R<sup>2</sup> of the 4 plots is 0.1. 410: Figure 8. Here you have something but you make no attempt in the Discussion to explain it. This result may reflect an expected chemical weathering threshold around zero degrees. Increases in winter temperatures, such that the soil is unfrozen for longer, is likely to be important in Swedish forest soils and your results support this. In general, with respect to the figures explain how you calculated your statistics. The equations are all expressed in terms of X and Y but the parameters vary between plots, so should be individually defined for each plot. R<sup>2</sup> should be R<sup>2</sup> (superscript). We will take these comments into account as we revise the results and discussion, and include a conclusion.

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