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

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Anonymous Referee #2 Received and published: 3 April 2019

General remarks: The manuscript describes how weathering rates may change under different climate projections in future. It uses a modelling approach (ForSAFE) on 544 forest sites in Sweden. One has to assume that a huge dataset is available that is used for modelling (but there is no data repository given). In my opinion, the manuscript is in a pre-mature stage and I cannot recommend it for publication. The manuscript is presented more like a technical report and I do not see much added value for the scientific community right now. Furthermore, similar studies have been carried out

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using another model that finally gave quite similar results. So, the question arises: why doing this exercise again? The used model in this study seems to be “a fully dynamic ecosystem model” – but it provides the same results.

Response to the reviewer’s general comments: This is a valuable comment as it questions the very purpose of the paper. Assuming that the reviewer refers to the earlier study by Akselsson et al (2016) which uses PROFILE to calculate weathering rates, our aim was to either confirm or question the conclusions of that study by explicitly taking into account seasonality and ecosystem feedbacks (particularly between soil and biota). In this respect, we hoped to move forward with the estimates as PROFILE is heavily dependent on user defined assumptions for uptake, litterfall and mineralisation, and soil moisture content and hydrological transport. Also, being a steady state model, it does not consider temporal dynamics. ForSAFE addresses these issues by internally calculating growth, uptake, litterfall, mineralisation and how these are controlled by soil chemistry and hydrology. It also explicitly simulate hydrology and heat transfer, allowing us to simulate the diffusion of air temperature into the soil, and responding to seasonal variations in precipitation, vapor pressure difference and evapotranspiration. That the results of this study agree with those in the work by Akselsson et al. is welcome, but the extent of the climate impact was shown here to be lower than expected earlier. Present weathering rates estimated by ForSAFE agree very well with those from PROFILE. Yet the estimated impact of future climate calculated by the two models is different for the different climate scenarios (+23% vs. +33% and +26% vs 20% from ForSAFE and PROFILE, using CCSM and ECHAM respectively). The two models simulated different periods, but when looking at the increase of weathering per degree increase in air temperature, ForSAFE give a significantly lower estimate. What we want to point out is that considering ecosystem dynamics, together with seasonality, can significantly impact our estimates of the effects of climate change on mineral weathering. The reviewer’s critical response is very valuable for making us aware that this message maybe did not come through. We would like to reorganise the paper in two ways to bring the paper up to the requested level: 1- a more comprehensive

section for materials and methods, including an evaluation of the model, and 2- an overhaul of the results and discussion sections (also in accordance with reviewer 1) to present more dynamic process results and lift more the difference between this study and previous work.

I see the following major flaws: - no link to a data repository or at least to a summary of the data is given (e.g. as supplementary material) We will revise this and provide a detailed summary of the data used, rather than referring to other work.

- I would have expected at least a sensitivity analysis of the main parameters used. I see nice maps – but have no idea how reliable or sensitive the projected results are We are uncertain if a sensitivity analysis is really imperative here. Unlike PROFILE, we do not need to assume things like soil moisture in ForSAFE. Sensitivity analyses of specific parameters (eg. controlling hydrology) have been done in other studies (eg. Zanchi et al., 2016; Kronnäs et al., this issue). We will however include a clear assessment of model output against available empirical data.

- what is the novelty of this research? The novelty of the paper is in considering internal ecosystem feedbacks and high temporal resolution is forecasting the expected gain in weathering from climate change. This, in comparison to a steady state model (eg. PROFILE in Akselsson et al., 2016), limits the increase in weathering per degree of temperature from around 10% to below 8%. Again, thank you for making us aware that we failed to communicate the this.

- no overview of the present state of knowledge and gap in knowledge is given. There are other models and approaches. I would like to see advantages/disadvantages of existing approaches. Other modelling approaches are more physically based, e.g. the percolation theory (see e.g., Hunt and Ghanbarian, 2016). Consequently, I would expect that the theoretical concept is much better embedded to demonstrate why now a modelling exercise using ForSAFE is necessary to be performed. The approach in this study is indeed different from that in Hunt and Ghanbarjan. We do not really

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understand what the reviewer aims at in the last part this comment. The first part however would certainly give more context to the paper, and we will include a short but up to date background section on existing approaches. The purpose of this paper not being an assessment of approaches, we do not clearly see the need to motivate the choice of model more than the requirement for process and temporal dynamics on a decadal scale.

As previously mention, my recommendation is to reject this manuscript. A fully rewritten and reorganised paper can be reconsidered. We would like to revise the paper as described in the responses.

Details: L. 45: Ref.? Thank for pointing out the omission. We will refer to the relevant source.

L. 49-54: relation to climate? Iwald et al. showed no relation to climate. The study is referred to here to show that base cation budgets are very tight, reducing the error margins we can accept in estimates and requiring as precis weathering estimates as we can get.

L. 74-77: should be extended. There is not only the programme ForSAFE. What about others? Better overview of current knowledge. Derive research questions. Yes we agree with the reviewer, we will include a more extensive review of other methods. We will also try to formulate specific research questions that will lead to the aim.

L. 126-144: what about a sensitivity analysis: atmospheric deposition, forest management, . . . A separate study by Kronnäs et al looked more at these aspects and how ForSAFE responds to them. We do not necessarily see the need for these here, as we try to focus on climate.

L. 170-171: make reference to table We will refer to the table earlier in this paragraph.

L. 182: how is the increase in soil temperature calculated? Which soil depth? L. 209: how is soil moisture modelled? Which soil depth? We realise these are aspects we

did not describe. We will add a section in the methods describing the modelled soil depths, and how the model deals with soil moisture and temperature. Because the model simulates three to five layers at each modelled stand, we show the cumulative weathering rate for the entire profile, but only soil temperature at the middle of the profile (i.e. at half the simulated depth, which usually is between 40cm and 50cm depending on the site).

L. 225-228: . . . so, why doing this exercise? There seems no difference to PROFILE which does not seem to be surprising because it is the basis of ForSAFE (see L. 89). The mineral dissolution equations are the same between PROFILE and ForSAFE. The difference is that PROFILE takes all other fluxes to and from the soil solution as inputs (uptake, mineralisation, water percolation. . .) and considers no cation exchange. ForSAFE takes only climate, atmospheric deposition and forest management, while all other processes are modelled internally. PROFILE produces steady state weathering levels, while ForSAFE allows us to follow the change over time, with sometimes significant delays in response as the ecosystem adapts to new conditions. Most importantly, and the reason why we needed this study, is that ForSAFE can account on its own for the feedback between plants and soil. As it gets warmer, more water will be lost because of evapotranspiration, and this effect is even more expressed during the growing season. Akselsson et al shows present weathering rates at selected locations, while here we try to capture current (and double check them with the literature) and future weathering rates, and we believe a tool like ForSAFE is suited for that.

References: Hunt, A. G., and Ghanbarian, B. (2016). Percolation Theory for Solute Transport in Porous Media: Geochemistry, Geomorphology, and Carbon Cycling. Water Resour. Res. 52, 7444–7459. doi: 10.1002/2016WR019289

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