

Interactive comment on “Weathering rates in Swedish forest soils” by Cecilia Akselsson et al.

Cecilia Akselsson et al.

cecilia.akselsson@nateko.lu.se

Received and published: 3 May 2019

Referee 1 starts off with a paragraph with general comments, followed by a long paragraph with specific comments. Below we list all comments, followed by our answers. We also attach a pdf with this text, but formatted in a way so that it is easier to separate between comments and answers.

General comments The authors present an overview or synthesis of base cation weathering studies carried out under the Swedish QWARTS project. The paper is clearly part of a special issue, as much of the text refers to other papers in ‘this issue’. Given the dependence on other papers, it is not a standalone paper but a very ‘Swedish’ view of soil weathering. Nonetheless, the paper has an important objective, to demonstrate that despite the variation in estimated soil base cation weathering rates at the site level, there is general agreement and these data can be used to support the assessment of

C1

sustainable forestry. However, the paper falls down in several areas: 1. The text is overly long, at times there is extensive repetition within and between sections, We will shorten the paper by removing redundant and repetitive text and shorten parts that are described in other papers in the special issue. The main changes are listed below: We will change to a more conventional structure with a Methods section and a Result and discussion section. This will eliminate much of the repetitions. This is further described in the answer to general comment #2 below. We will remove descriptions of models not represented in the results, according to the answer to general comment #2 below. We will rework the chapter “Potential for biological weathering”, so that the main conclusions from the paper about biological weathering are highlighted and details are removed, according to the description below. This will shorten the chapter by about 30%. We have also described that in the answer to the Editor, and therefore the following text overlaps with parts of the answer to the Editor. The main measures are: -Shortening of the introduction in the first paragraph by removal of the three first sentences on p14, line 19-22. -Removal of details about the capacity of EM fungi to allocate C selectively to different minerals, by removal of 2 sentences starting on p15 line 34, describing results in three studies (Smits et al., 2012; Leake et al., 2008; Schmalenberger et al., 2015). -Removal of redundant first sentences in paragraphs starting on p 16, line 17 and 25. -Removal of paragraph starting on p16, line 33 and ending on p17, line 6, about the study by Smits and Wallander showing the effect of vegetation on apatite weathering. It is enough to mention that the effect of vegetation is partly but not fully built in to the models in the following paragraph. -Removal of the last two paragraphs, starting on p17, line 26 and ending at p18, line 9, which are based on on-going work, and thus would have needed even more explanation for the reader to be able to evaluate it. We will shorten the chapter about Future research, according to the answer to general comment #3 below. 2. The text had a tendency to loose focus, the manuscript jumps between project summary, scientific review, and comparison of scientific results, and while the authors forewarn of the contents in the abstract, the conclusion more succinctly speaks to the true contribution of the paper, if there are

C2

other papers in this special issue, do the authors need to be so broad in their coverage? This comment, as well as general comment #3, questions the structure of the paper. Based on these comments, and recommendation 'a' (below), we will clarify the aims, restructure the text, remove parts that are not contributing to answering the aims, remove repetitious text and shorten parts that are described in other papers in the special issue. This restructuring and shortening of the text is described more thoroughly below, and in answers to general comment #1 and #3. -As the referees point out, an important aim of the paper was to investigate the variation in weathering rates from different approaches, review the results with the key uncertainties for each method in mind and assess the robustness of the results in relation to sustainable forestry. However, a second aim of the paper, based on the research progress within the QWARTS programme, was to highlight potential implications for these results of new in-sights regarding two mechanisms identified as potential sources of errors in modelled weathering rates: lack in the representation of biological weathering as well as overly simplified descriptions of base cation exchange and aluminium complexation. A third aim was to propose future research to further reduce uncertainties, also based on insight from the QWARTS programme. We will clarify the aims accordingly in the end of the introduction. -Based on the aims, we don't want to completely remove the parts about the results from the other papers in the special issue, since they contribute to aims #2 and #3. However, we agree that those parts could be substantially shortened, in order not to overlap too much with the other papers in the special issue. -We will change the structure, so that it follows a more conventional scheme, with a Methods section and a Results and Discussion section. We will start the Methods section by describing how we have compiled the data for the weathering rate comparison based on literature studies, we will continue with a brief description of each method (shortened versions of the present ones), including methods for regional applications. Finally we will describe how we made the comparison with harvest losses. We will remove the descriptions of the models not used for any of the sites in the paper, i.e. WHITCH and Crunchflow. By having a pure Methods chapter, we will avoid some of the repetition that now exists, e.g. regarding

C3

model descriptions (in the chapter "Methods for estimating weathering rates" and the chapter "Weathering rate comparisons on a regional scale"). In the Results and discussion chapter we will start with site level results, continue with regional results and then the comparison with harvest losses. Thereafter, sections about potential for biological weathering, more detailed reactions and future research will follow. 3. Section names and section contents are confusing, the section on future research seems to focus on limitations, while repeating text from previous sections, and generally has the feeling that much of the text could have been integrated into previous sections- Regarding confusing section names and contents, see answer to general comment #2, where we describe how we will restructure the paper with new section names. We agree that the section about future research is too long and insufficiently focused. We will shorten it substantially, so that only the parts describing actual future research are kept. We list our major changes below: -The first paragraph will be replaced by one sentence which introduces the chapter. -The chapter "8.1 Model development: Biological weathering" will be more straight forward towards actual model development. Thus, the first paragraph will be removed, so that the chapter starts with the sentence "Existing models. . .". Minor adjustments are required in that paragraph, since it refers to the paragraph that will be removed. The last paragraph will be removed. -The chapter "8.2 Model development: Higher resolution chemical reaction" is repeating some of the results presented in chapter 6. The repetitive text will be removed (large parts of the first paragraph). -The chapter "8.3 Model development: Implementing weathering brakes" will be shortened, by removing the first part, going directly to the actual work that needs to be done to better model the saturated zone. -The chapter "8.4 Model development: Weathering below the root zone – for surface water quality assessments" will be shortened by removing most of the first paragraph, jumping directly into the further testing and improvement of the ForSAFE2D model. -In the chapter "8.5 Reducing uncertainties in model input data", a few parameters that are of key importance but where the uncertainties are large, are gone through. This chapter will be shortened by removing much of the text describing results from a number of studies, and instead

C4

going directly in to what needs to be improved. -The first paragraph of the chapter “8.6 Comparison between modelled weathering and other estimates of weathering” will be removed. The last paragraph will also be removed, but parts of it will be incorporated in the chapter where the results from the site level approaches are presented. 4. Unfortunately, much of the comparison between weathering estimates is too qualitative, there is no quantitative assessment, statements such as ‘they agree’, ‘do not agree’ or ‘estimates are similar’ need quantitative support. We will add statistical measures, i.e. median and interquartile ranges (or average and range in the case of only two estimates), for each site to the table where the weathering estimates from all single sites, according to the different approaches, are presented (Table 1). We will refer to those measures in the text. Moreover, we will develop statements like ‘they agree’ or ‘they do not agree’ so that they reflect the difference quantitatively, by adding information about how much they differ (in %). We will also add a Table, in conjunction with Figure 5, statistical measures of : [harvest losses after with – weathering] for each of the sites in Figure 5. By doing that we can differ between sites where the results are more or less robust. I suggest the authors (a) step back from their manuscript and try to pinpoint their exact (unique) contribution, (b) they should remove repetitious text, and remove text that is described (reviewed) elsewhere in the special issue, and (c) add a stronger quantitative element to their comparison / assessment of weathering / sustainable forestry. (a) See answer to question #2 above. (b) See answer to question #1 above. (c) See answer to question #4 above. Specific comments Page 2 L1. It was internationally recognised during the 1970s but regionally recognised long before that... 1 to 2 decades! We will change from “first recognised” to “internationally recognized”. L2. one could argue that the peak was a little later... 1980s to 1990s? We will change to “1980s to 1990s”. L4. Reword / clarify ‘more harvest’, more correctly you are referring to the use of forest residues for renewables! We will replace “harvest” with “forest residues” L7. lab → laboratory We will change accordingly. L7. There was no intensive modelling? Perhaps ‘extensive’ is superior? Yes, we will remove “extensive”. L8. Simplify (here and throughout): ‘This paper presents the state...’ We

C5

will make clarify the aim based on the general comments of Referee 1, as described in the answers to those comments above. We will try to simplify them according to this comment, when we do these changes. L9. You jump too quickly into the specifics of the results, give the reader a more guided introduction, ‘Under the project, we found that...’ We will start the presentation of the results with the suggested phrase. L10. Variation from what? Data? Methods? Remember the international audience knows nothing of the project! We refer to the variation in estimated weathering rates from different approaches. We will clarify that. L12. Important but the manuscript would greatly benefit from the ‘word smiting’ of the native English-speaking co-authors, Finlay and Bishop? The manuscript was corrected by a professional language editor before submission. Bishop will be deeply involved in the review work, which will ensure the quality of the revised text. L13. I think this is an important result but the term ‘clear imbalances’ obscures the implications of the findings. The activities are unsustainable. We will clarify the text: “. . . showed sites where whole-tree harvesting was clearly not sustainable. . .”. L16. Step back and provide greater support... approaches based on the weathering of (observed) mineralogy, such as PROFILE..., provide the most important fundamental understanding of the contribution of weathering to long-term availability of base cations to support forest growth, nonetheless, these approaches should be continually assessed against...’ We will change the text based on the suggestion. L19. this point needs further development / clarity The two last sentences will be modified: “Uncertainties in the model approaches can be further reduced, mainly by finding ways to reduce uncertainties in input data on soil texture and associated hydrological parameters, but also by developing the models to better represent the delivery of weathering products to runoff waters and biological feedbacks under the influence of climate change. Another future research activity will be to develop the models to better represent the delivery of weathering products to runoff waters,” Page 3. L1. change acid to acidic throughout We will go through the document and change accordingly. L3. remove one ‘processes’ We will reformulate to: “. . .extensive research examined processes that acidifies and counteracts acidification. . .”. L5. refer to SWAP first, it

C6

started before NAPAP (and is more important in a European context) We will change the order of SWAP and NAPAP. L7. You need to provide more context for critical loads; it is an effects-based approach for emissions reductions, essentially a direct response to the recognition that emissions of sulphur dioxide were causing significant impacts. Notably it has nothing to do with SWAP or NAPAP! We will develop the part about critical loads accordingly. L9. 'A critical load...' We will change from "The.." to "A..". L11. '... critical loads of acidity...' We will add "...of acidity". L15. Yes, very true but those of us interested in water barely consider weathering directly...? We don't understand what Referee 1 suggests here. We have not done any changes. L16. '... and as such a sink of acidity'. We will revise as suggested. L19. You need to differentiate between plot and catchment scale estimates, models such as MAGIC are process-based, and can be used to provide catchment-based estimates of weathering, however, they are fundamentally different to process-based estimates from PROFILE. We will clarify that the different approaches are addressing different scales. L24. I do not completely agree with this. Many jurisdictions were faced with national scale modelling, and the application of simple approaches such as 'skokloster' provide a practical solution compared to the application of a process-based model that requires quantitative mineralogy on a high spatial resolution... more correctly, given the high loads at that time, the uncertainty in weathering was trivial. We agree that the uncertainties were less important in the times of high loads, which affected the weathering research. We will add a sentence about that. L29. Was it severity, or a shift in policy to support mitigation of climate change impacts? The policies changed when the severity of climate change became fully recognized, and it was the policy shift which led to higher demand of renewable fuel. We will change the text accordingly. Page 4. L1. increase from 25 Yes it increased again after 2016, it was a temporal dip. We will clarify that. L3. Substantial Will be corrected. L5. Depletion methods needs more description... or just exclude such detail for the moment (estimates of base cation weathering...) The description of the depletion method comes later. We did not do any changes. L11. 'Akselsson et al. (2007) used a mass balance approach (with weathering estimated using PROFILE) ...' We will

C7

change accordingly. L17. I suggest 'Similarly, the influence of whole-tree harvesting ...' Yes, we agree and will change accordingly. L20. Yes, but only in a Scandinavian context, this has not spilled over into the rest of Europe or north America (yet). OK, it is interesting to think about why. We will add "in Scandinavia". L23. Was the conclusion valid? I would suggest the greatest uncertainty was derived from comparing approaches that should not have been compared? We agree that it makes no sense to compare weathering rates estimated for different soil depths. That is also what Futter et al concluded, which we write about in the next paragraph. We have not done any changes here. L30. It is okay to call out errors. Three approaches? In truth there are two. Mass balance approaches where you indirectly estimate weathering rate (there are also other indirect methods) OR mineralogy-based approaches, often if mineralogy is not available you have surrogate-based approaches but 'at least three approaches' may verge on ridiculous? Weathering is the breakdown of minerals... so what does three independent approaches refer to? We are also skeptical about the conclusion about three approaches. However, we have written about that in the discussion, and base it on the results from this study. We think that the discussion is a better place to "call out errors" than the introduction.

Page 5. L1. Provide background on the depletion method... total analysis regression... the reader need help. We write more about this later, we don't want to make the introduction too long. However, we will with a few words describe each method also here. L3-4. reference to other methods are difficult to navigate... We will put each reference directly after the method that they refer to. L7-8. Combine sentences... reduce words... We will shorten the part about the two sources of uncertainties. L17. '... by revisiting older w...' Will be corrected. L21. Flows or removals? Removals is better. We will change accordingly. L30. Replace 'Cows' with 'sources and sinks' We will change to 'sources and sinks'. Page 6. L2. Cite 'Warfvinge and Sverdrup, 1992' for PROFILE OK, we will do change to that reference. L2. The work of Susan Brantley should be cited here We will add a reference to Brantley. L7. The key point here, and what separates PROFILE from other approaches, it that weathering is derived from

C8

the breakdown of mineralogy (an essential input), the other inputs only estimate the amount of minerals that are being weathered. We agree and will clarify that. L13. Again, it might be worth citing Brantley here... We will do that. L17. hydrological model... Will be corrected. L21. The discussion / details on SAFE and ForSAFE can be removed. We agree and will remove this. L30. Simplify to PROFILE This section will be removed, as part of the shortening of the paper. See answer to general comment #2.

Page 7 L4. There is an application of SAFE to Hubbard Brook which models the catchment (compared with MAGIC, VSD, etc.) Also this section will be removed or shortened, we will focus on the methods used in this study, in order to get a shorter and more focused paper, according to the reviewer comments. L7. Should MAGIC be cited here or under 'mass balance' approaches? PnET-BGC is another example of a model that uses a mass-balance approach We actually thought a lot about that. It fits in both chapters. We have thought about it again and will put it under mass balance approaches. L17. Assumes that deeper soil is the parent material, so does not work for glaciofluvial soils, etc. We will clarify that in assumption nr 2 on row 23. L26. Could add that the approach has been widely used and cite a few examples? We will do that. L27. Typically referred to as 'Catchment mass balance budgets' as they are widely estimated at the catchment scale, as such the estimates of weathering are an average of a larger landscape unit and can be highly influenced by localised geology. We have examples of both catchment mass balance budgets and site mass balance studies in this paper. The weathering assessments in Asa and Flakaliden are site based. Therefore, we will not change to 'Catchment mass balance budgets'. L28. MAGIC should really be mentioned in this section! See answer to comment on L7 about. Page 8 L10. Retitle to 'strontium isotope ratio' We will. L17. This really should be included under the depletion, as it is a derivative of that approach We agree and will merge those chapters. We separated them because they have been presented as different methods before, but we agree that it is confusing. L24. Remind the reader that you are focused on QWARTZ, i.e., 'Under QWARTZ, weathering ...' We will change

C9

accordingly. L30. 'profile 17-20cm deeper', this is a little unclear. I assume in simple terms the soil depth differs between estimates... Maybe present table on a 'weathering per cm'? We will simplify to "...the highest rates in the interval are associated with the deepest soil profiles." We don't want to add a new table, since we are trying to shorten the paper, and since the depths are the same for the same site except for in Gårdsjön and Svartberget. Page 9 L2. This is an important point and should perhaps be stated much earlier, homogeneity of soil and bedrock are important considerations for agreement / lack of agreement between approaches We think it fits good here, in the very first part of the results. L15. Why does the depletion indicate a lower weathering rate? This could suggest that the un-weathered layer did have weathering? In many of the studies in Table 1, the depletion method is lower. Why? However, it may also be argued that the range between methods is smaller than the uncertainty? We have some potential explanations to this in rows 27-31. One of the is according to the suggestion above about that the "original till" is actually weathered till from earlier glaciations. L21. Do you mean 'soil bulk density'? Yes, we will clarify that. L23. Correct 'to to' We will remove one 'to'. Page 10 L19. Disqualified? Excluded! However, just exclude, and note in a footnote to the Table. No need to explain We will change to 'excluded' and refer to Stendahl et al. instead of explaining. However, we keep it here, not in a footnote in the table, since the sites are not part of the table, and a footnote about three other sites feels a bit out of place. L30. Till is the most obvious / likely explanation... remove other excludes / reasons and only present this one. We will change the order, so that the till explanation comes first. However, we keep the others as they also might be contributory explanations. L34. What does 'conceptual limitations' mean? clarify We agree that this is not a good expression. We will clarify, based on the discussion in Stendahl et al. (2013). Page 11 L1. 'for Ca and Mg...' We will add 'for'. L3 to L10. This can be reduced to one sentence, state 'The majority of weathering rate estimates were classified as acidic or intermediate (cite Table, UNECE, etc.). We will reduce the text accordingly. L4. Figure 2 essentially repeats Table 2; the classification could be added to the Table 2 instead. Is the suggestion to

C10

remove Table 2? We think that it gives a good overview, much better than the tables, so we would like to keep it. L10 to L14. This text repeats detail already discussed. We will remove repetitious text here.

L20. This is a long section, and rather than go through each site (one by one), it would be more efficient to summarise and focus on the broad agreement, and disagree, but describe from the point-of-view of the factors that drive disagreement, e.g. '... there was slight disagreement between some estimates owing to difference in input data use by the different approaches, such as soil depth (give example) or soil moisture (give example). The table is provided, so the reader can evaluate the results, and there is no need to describe in detail. We will rework and shorten this section, according to the suggestion. L22. are they scaled-up or just regional applications (more sites)? How are they scaled? It is more regional applications (more sites). We will clarify that. L25 to L33. The relationship between ForSAFE and PROFILE has already been described at length. There is no need to repeat again. This will be solved by restructuring the paper to a more conventional structure with methods, results and discussion, see more detailed description in the answer to general comment #2. Page 12 L7. Is the analysis really only based on 11 sites? Did the study use 11 sites to predict at 400+? Yes, this is described in Olsson et al. (1993). Page 13 L5. Is this the same approach as used with PROFILE? Did both use UPPSALA? Clarify No, for PROFILE another normative method was used. We will add information about that. L14. Again, is this similar to MATCH used in PROFILE. Perhaps have a consistent description (and only described in one place in the text) Yes, MATCH was used for both. We will restructure so that all method descriptions are in one place, see answer to general comment #2. L15. Why compare PROFILE and ForSAFE. Are they dramatically different, or are you just comparing the effect of different hydrological data on estimates of weathering? The justification for this needs to be clearly stated under section 4. This is a very different comparison to total analysis (which is fundamentally a different estimate of weathering). We agree that the comparison is very different than a comparison with to completely different approaches. PROFILE and ForSAFE are built on the same

C11

weathering reactions, but the dynamics in ForSAFE affects weathering rates. Also, the fact that e.g. hydrology is modelled in ForSAFE, whereas it is input data in PROFILE, may affect weathering rates. This is described thoroughly in Kronnäs et al. in the same issue. We will clarify this, and also explain why we want to include both PROFILE and ForSAFE in the Methods part of the paper. L16 to L18. This detail should be presented under the main part of section 4 This will be solved with the new structure with a methods section (see answer to general comment #2).

L19. Why use climate regions? We want to show regional differences, and those regions are logical in the way that they represent different climate and atmospheric deposition regimes. They have been used in one paper, just accepted for publication, and are used also in Belyazid et al in this issue (under review). L24. Above you have noted that the differences in estimates of weathering is often driven by differences in inputs (under different applications) for the same model. Here you add further confusion to that issue... What is the goal of the comparison? The goal of the comparison was not to run different approaches on many sites with exactly the same input data. The goal was to collect existing weathering estimates from different approaches, in many cases run independently of each other, and compare, to get a span covering both differences in approaches and differences in input data used. This represents the reality for e.g stakeholders. We show that, even though we do like this, we can draw conclusions about sustainability. We will, when we rework the aim according to the answer to general comment # 2, clarify this. L29. Above you state they are more-or-less the same, and since that statement we find that one is higher than the other, and so on... which is the truth? In the beginning of the chapter we write: "The weathering rates varied widely within the regions for all methods, but there were no large systematic differences between the medians or ranges for the different methods. However, ForSAFE gave somewhat higher medians than PROFILE for all regions, especially in the northern regions." On L29 we write: "The total analysis regression method gave somewhat higher weathering rates than PROFILE for several of the regions." We don't think that this is contradictory, but to clarify we will move the

C12

sentence about that total analysis regression gave somewhat higher weathering than PROFILE to the beginning of the chapter, right after the sentence about ForSAFE. L33. Maximum weathering depth? This is confusing, why compare if they represent different depths / pools? This needs clarification, the text suggests that PROFILE covers a shallower depth compared with Total Analysis. Why compare? Normalise both to the same depth before comparing. In this paper we have used results already published. We have decided not to try to normalize them, then we would add extra assumptions (e.g. that weathering per cm is constant in the soil profile, which we know is not true). We know e.g. that the weathering rates according to the depletion method are much slower further down in the soil profile. We have thus decided to discuss the differences in depths instead of normalizing. We will try to clarify that when we rework the aim (see also answer to comment to L24 above). Page 14 L6. Reword... what comparison between regions? Here we mean the comparison of weathering rate intervals between different regions. We will clarify this. L8-L9. This statement, and similarly many of the statements in the previous section, are very qualitative. There is no quantitative element to the comparison at the site or regional level. Statements such as broadly agree, similar / non-similar are fine IF they are also supported by a quantitative assessment. This is missing. See answer to general comment #4. L10+. This has been already stated, and is obvious, it should be noted in the main part of section 4, and not included as part of the comparison (it was known before starting the comparison). However, it would be useful to know the purpose for such a comparison? This will be solved by restructuring according to the answer to general comment #2. About the reason: see answer to comment L15 and L24. L15. I think (more-or-less) the results of this assessment are stated here, as such, perhaps the whole assessment could be collapsed to one paragraph? The results from site-level and regional applications will be lumped together in one results chapter, and yes, it will be substantially shortened. L27. If it is already described, then why repeat here? Repetitious text will be removed. L30. wording 'dependent' We will reformulate. Page 15. L1-L5. Citations? References will be added. L11. The weathering process

C13

in safe is not directly affected by biological processes, it is only affected in as far as the recognition that some of the processes are likely influenced by biology. Biological processes do affect weathering in PROFILE, SAFE and ForSAFE through the following pathways: 1- uptake reduces cation concentrations, directly alleviating the brakes on weathering, 2- transpiration reduces water availability, thus limiting one of the four weathering kinetics (dissolution in water), and increasing concentrations, which can have positive (in case of acids) or negative (in case of cations) effects on weathering, 3- cation uptake produces more protons in the soil solution, lowering pH and promoting weathering, and 4- the production of dissolved organic radicals through litterfall decomposition and in the case of ForSAFE exudation contribute directly to one of the four weathering kinetics. Through these four pathways, the models do directly modify weathering rates based on the outcome of biological activity as described here. L12. PROFILE has some biological feedback? Really? See answer to L11. L33. 'There is extensive literature...' We will remove "an". Page 16. L1 to L34. The entire page (and some of the previous) presents a good review of the 'state of knowledge' but it can be much reduced... and the benefit / objective of such a review should be considered... why cover so much text if this is not part of the work under QWARTS described by the authors. We will shorten this chapter, see answer to general comment #1. Page 17. L24. 'We found'? Which 'we'? We will reformulate. Page 18. L4. Was this stated already? The chapter will be substantially shortened, we will make sure to get rid of repetitious text. L10. What does the section title mean? We will try to simplify the title. L17. Why 'state-of-the-art'. WHAM has been around for decades? We will remove 'state of the art'. L22. Replace 'former' with specific term. OK, we will do that. Page 19 L10. I wonder if this is some of the context for this manuscript that would be better to present at the start? We don't understand what the suggestion is here. L20. 'In regions where weathering rates...' We will change accordingly. L23. Clarify or reword 'data on site index could be found' We will change to "site quality". Page 20 L7. Differences in weathering has already been discussed? We will shorten the text about this, but we need to mention the differences also here,

C14

since the sites with large differences are the ones where it is more difficult to draw conclusions about sustainability. L10. This sounds like it was stated already? No, this is the first time we compare weathering and WTH in the north. However, we will restructure and shorten the text to make it easier to follow. L21. The preceding text could be summarised much more succinctly. See answer to comment L10 above. L31. This is more-or-less the summary of the results (if a further quantitative description was added) then this would be sufficient. See answer to comment L10 above. Page 21. L12 to L18. This has nothing to do with future, it is mostly repetitious text. See answer to general comment # 3. L22. This section seems odd... we have just been presented with a 2+ page section that covered biological weathering. It is difficult to justify this additional text! I believe this section should be removed, and any 'fresh' text be included above. We will shorten the chapter substantially, according to answer to general comment #3. L23. Yet despite the previous lengthy discussion on biological weathering, we were not introduced to the term 'EPS'?? This will be looked over when we do the comprehensive revision of the biological weathering part. L23 to L4 Page 22. This text can be deleted. We don't understand which text Referee 1 refers to, it seems to be something wrong with the line numbers (L23-L4?) Page 22 L5 to L11. Is this model development or uncertainties? It is a lot of uncertainties. We will shorten the chapter substantially, according to answer to general comment #3. L24. Again, it is difficult to justify such an extension section, shortly after we have already been presented with a discussion on the topic. This will be shortened, to only contain the main model improvements required. L25 to L32. This is not model development...just repetitious text See answer to L24 above. Page 23 L1. This is a trivial point, with the right implementation the speed can improve. Just because it takes an hour to cycle to work, does not mean that everyone must cycle! The organic complexation does not have an arithmetic solution, the only way to do it without compromising its purpose is by optimization (of multiple complexation parameters) and a whole set of assumptions (including that the Al pool is constant for example), i.e. iterations until near-equilibrium. In HD-Minteq this is feasible because most state variables and fluxes

C15

(soil organic matter, decomposition, uptake...) are given as input and therefore not dependent on changes in soil solution chemistry. While in ForSAFE these processes are internally simulated, meaning that each iteration of Al complexation will require an entire simulation of all fluxes and equilibria, exponentially increasing computation time or even requiring a new optimization of exchange parameters, which in the word of the original author "is impossible" (see Gustafsson 2001, Journal of Colloid and Interface Science 244, 102-112. doi:10.1006/jcis.2001.7871). L8 to L11. I am sure this is repetitious text. We will remove this. L14. 'overestimated estimates of weathering rates' -> 'overestimated rates of weathering' We will change accordingly L19. So it was not tested? Is this future research? It seems to be more of 'ongoing' research? New brakes have not been implemented and tested in PROFILE/ForSAFE. This is future work. In Erlandsson et al., we implemented and tested silicate release through weathering and the feedback brakes from Si concentrations on the four weathering kinetic equations, while dictating other inputs such as uptake, water flow and so on. This has produced the expected results (constraining weathering in the saturated zone). Implementation in PROFILE/ForSAFE has not yet been carried out. L20. Is this catchment scale weathering? Yes, we will call it that instead. L21 to L27. This is not future research, this is improvements needed in the application of weathering models (and better linkages with hydrological models). As such, uncertainties or limitations is a more appropriate section. We will, in the restructuring, divide the chapter called "Future research" in a chapter called "Model Limitation/Uncertainties" or similar and one chapter called "On-going and future research" or similar. Both will be part of the "Discussion" chapter. L28 to L4 Page 24. Not future... ongoing /current research? See answer to comment L21-L27 above. Page 24 L5. See comments above... much of the text presented so far under 'future research' speaks more to limitations in application or uncertainties. See answer to comment L21-L27 above. L6. This is PROFILE specific text... not the depletion method, or others... Yes. We will clarify that in the title. L8. Often? Often will be removed. L9. 'Not only are...' We will correct this. L13 to L20. This is a very uncertain uncertainty... why so much space for something that

C16

is not 'very uncertain'? It is part of the QWARZ work and refers to one of the papers in the special issue. So we want to refer to it, but we will shorten the paragraph. L24. Unless that span is used to estimate the uncertainty... It is in one of the papers of the special issue. But still, often one value is used. L26. This does not make sense. Is it possible to contain the solution space? The reviewer is right that this is not a problem if we have information on which minerals are present, their respective stoichiometries and in what proportions (See Posch and Kurz, 2007). This however is most often missing, and that is the purpose of using A2M. It would, as indicated, be possible to narrow the space in more information is available, on for example the fraction of light primary minerals in the soil. So yes, it is possible to constrain the space if a considerably more information is available. L28. This is not true. However, many users make that assumption. However, others do not. In the cases we have seen, where one of the mineralogies is chosen from A2M, it has been the centre point, although everyone who uses A2M should know that all solutions are as likely. We think that we have been clear about that all have the same probability, but that many chose the center point, since they want one value. Page 25. L1. BET may still be the best technology? We will remove "using modern technology". Even if the BET technology hasn't developed since the 80/90:ies (which we don't know), the regressions could be revised based on a larger soil material. L2. Are the 'current uncertainties (?)' quantified? Are there uncertainties? The regression graphs in Warfvinge and Sverdrup (1995) indicate large uncertainties. We will clarify that. L5 to L9. Repetitious text. When restructuring, we will make sure to only write about this once. L10. Improved soil moisture should come with improved soil hydrological modelling... Yes, and since this chapter is about improved input data we mention soil data which is very important for the hydrology modelling in ForSAFE. We don't understand what Referee 1 suggests here. L12. All estimates are modelled? What are the other? We will change to "Comparison of weathering estimates from different methods" or similar. L13 to L17 Page 26. This whole section can be deleted. Any useful should be moved to the section on comparisons. This is not 'future research' OK, this will be

C17

removed from here. L23. Manual? This is a bit trivial... delete and move to personal 'to do' list. I suggest you write a paper on this. OK, we remove this. However, it is very difficult to evaluate results from different depletion method studies, since it is so much up to the user to set e.g. the reference depth. It would have been helpful if a routine was developed, maybe even built in to a model. L28. They were not outliers. They represent measurements for different compartments. This is well understood. Mass balance methods are still used for weathering rate calculations and advocated by many. We included it here and came to the conclusion that it was problematic in the cases we had. We write "In the compilation of weathering rates in this paper, the most extreme outliers came from the budget method, which can be explained by the fact that other sources than weathering are included (Rosenstock et al., in review (this issue)). For a fair comparison between weathering rates from the budget method and from other methods, ways to distinguish between different sources need to be further developed." We think that this is a rather strong recommendation, which we would like to keep as it is. Page 26. L10. This is wrong. It is okay to state that. There independent methods? How many methods are there (truly independent)? More correctly, Futter et al. (2012) should have recommended that a method incorporating soil mineralogy be used (all other approaches are surrogates for weathering). We think that we are rather clear when we conclude that it is unrealistic. We think that it is stronger to say that, than to say that is absurd or wrong, which are more "value" words. L16. Good but you can more clearly call it out as an absurd suggestion. We think it is enough to say that it is unrealistic in practice. L22. Was some of this difference on single sites driven by differences in depths / inputs? Yes. We will change to "Although the variation in weathering estimates was large on single sites". By doing that we include methods as well as input data and to some extent depths. L21 to L30. I agree that these are the primary conclusions from this work; I would urge the authors to reflect on this when revising the manuscript. Much of the text can be reduced and streamline to better present this issue (conclusion). We will do this, see further answer to general comment # 1-3. Page 27. L10. Other approaches? Yes,

C18

the depletion method, mass balance calculations and other models. We will clarify that.

Please also note the supplement to this comment:

<https://www.biogeosciences-discuss.net/bg-2019-1/bg-2019-1-AC1-supplement.pdf>

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2019-1>, 2019.