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Interactive comment on "Weathering rates in Swedish forest soils" *by* Cecilia Akselsson et al.

Anonymous Referee #1

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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 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, (2) the text had a tendency to loose focus, the manuscript jumps between project summary, scientific review, and comparison of specific 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 other papers in this special issue, do the authors need to

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be so broad in their coverage?, (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, and (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. 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. I have no reservation in supporting the publication of the manuscript, however, I believe it currently needs major revisions before being accepted. However, much of these revisions only require reorganising, restructuring and rethinking. I have provided a number of specific comments below. Please excuse typographical errors.

Specific comments by Page and Line (L) number. Page 2 L1. It was internationally recognised during the 1970s but regionally recognised long before that... 1 to 2 decades! L2. one could argue that the peak was a little later... 1980s to 1990s? L4. Reword / clarify 'more harvest', more correctly you are referring to the use of forest residues for renewables! L7. lab –> laboratory L7. There was no intensive modelling? Perhaps 'extensive' is superfluous? L8. Simplify (here and throughout): 'This paper presents the state...' L9. You jump too quickly into the specific of the results, give the reader a more guided introduction, 'Under the project, we found that... ' L10. Variation from what? Data? Methods? Remember the international audience knows nothing of the project! L12. Important but the manuscript would greatly benefit from the 'word smiting' of the native English-speaking co-authors, Finlay and Bishop? L13. I think this is an important result but the term 'clear imbalances' obscures the implications of the findings. The activities are unstainable. 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 weath-

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ering to long-term availability of base cations to support forest growth, nonetheless, these approaches should be continually assessed against...' L19. this point needs further development / clarity Page 3. L1. change acid to acidic throughout L3. remove one 'processes' L5. refer to SWAP first, it started before NAPAP (and is more important in a European context) 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! L9. 'A critical load...' L11. '... critical loads of acidity...' L15. Yes, very true but those of us interested in water barely consider weathering directly...? L16. '... and as such a sink of acidity'. 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. 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. L29. Was it severity, or a shift in policy to support mitigation of climate change impacts? Page 4. L1. increase from 25L3. Substantial L5. Depletion methods needs more description... or just exclude such detail for the moment (estimates of base cation weathering...) L11. 'Akselsson et al. (2007) used a mass balance approach (with weathering estimated using PROFILE) ...' L17. I suggest 'Similarly, the influence of whole-tree harvesting ...' L20. Yes, but only in a Scandinavian context, this has not spilled over into the rest of Europe or north America (yet). L23. Was the conclusion valid? I would suggest the greatest uncertainty was derived from comparing approaches that should not have been compared? 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

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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? Page 5. L1. Provide background on the depletion method... total analysis regression... the reader need help. L3-4. reference to other methods are difficult to navigate... L7-8. Combine sentences... reduce words... L17. '... by revisiting older w...' L21. Flows or removals? L30. Replace flows with 'sources and sinks' Page 6. L2. Cite 'Warfvinge and Sverdrup, 1992' for PROFILE L2. The work of Susan Brantley should be cited here L7. The key point here, and what separates PROFILE from other approaches, it that weathering is derived from the breakdown of mineralogy (an essential input), the other inputs only estimate the amount of minerals that are being weathered. L13. Again, it might be worth citing Brantley here... L17. hydrological model... L21. The discussion / details on SAFE and ForSAFE can be removed. L30. Simplify to PROFILE Page 7 L4. There is an application of SAFE to Hubbard Brook which models the catchment (compared with MAGIC, VSD, etc.) 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 L17. Assumes that deeper soil is the parent material, so does not work for glaciofluvial soils, etc. L26. Could add that the approach has been widely used and cite a few examples? 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. L28. MAGIC should really be mentioned in this section! Page 8 L10. Retitle to 'strontium isotope ratio' L17. This really should be included under the depletion, as it is a derivative of that approach L24. Remind the reader that you are focused on QWARTZ, i.e., 'Under QWARTZ, weathering ...' 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'? 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 L15. Why does the

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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? L21. Do you mean 'soil bulk density'? L23. Correct 'to to' Page 10 L19. Disqualified? Excluded! However, just exclude, and note in a footnote to the Table. No need to explain L30. Till is the most obvious / likely explanation... remove other excludes / reasons and only present this one L34. What does 'conceptual limitations' mean? clarify Page 11 L1. 'for Ca and Mg...' 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.). L4. Figure 2 essentially repeats Table 2; the classification could be added to the Table 2 instead. L10 to L14. This text repeats detail already discussed. 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. L22. are they scaled-up or just regional applications (more sites)? How are they scaled? L25 to L33. The relationship between ForSAFE and PROFILE has already been described at length. There is no need to repeat again. Page 12 L7. Is the analysis really only based on 11 sites? Did the study use 11 sites to predict at 400+? Page 13 L5. Is this the same approach as used with PROFILE? Did both use UPPSALA? Clarify L14. Again, is this similar to MATCH used in PROFILE. Perhaps have a consistent description (and only described in one place in the text) 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). L16 to L18. This detail should be

presented under the main part of section 4 L19. Why use climate regions? 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? 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? 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. They why compare? Normalise both to the same depth before comparing. Page 14 L6. Reword... what comparison between regions? 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. 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? 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? L27. If it is already described, then why repeat here? L30. wording 'dependent' Page 15. L1-L5. Citations? L11. The weathering process 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. L12. PROFILE has some biological feedback? Really? L33. 'There is extensive literature...' 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 if not part of the work under QWARTS described by the authors. Page 17. L24. 'We found'? Which 'we'? Page 18. L4. Was this stated already? L10. What does the section title mean? L17.Why 'state-of-the-art'. WHAM has been around for decades? L22. Re-

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place 'former' with specific term. Page 19 L10. I wonder if this is some of the context for this manuscript that would be better to present at the start? L20. 'In regions where weathering rates...' L23. Clarify or reword 'data on site index could be found' Page 20 L7. Differences in weathering has already been discussed? L10. This sounds like it was stated already? L21. The preceding text could be summarised much more succinctly. L31. This is more-or-less the summary of the results (if a further quantitative description was added) then this would be sufficient. Page 21. L12 to L18. This has nothing to do with future, it is mostly repetitious text. 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. L23. Yet despite the previous lengthy discussion on biological weathering, we were not introduced to the term 'EPS'?? L23 to L4 Page 22. This text can be deleted. Page 22 L5to L11. Is this model development or uncertainties? L24. Again, it is difficult to justify such an extension section, shortly after we have already been presented with a discussion on the topic. L25 to L32. This is not model development...just repetitious text Page 23 L1. This is a trivial point, with the right implementation the sped can improve. Just because it takes an hour to cycle to work, does not mean that everyone must cycle! L8 to L11. I am sure this is repetitious text. L14. ' overestimated estimates of weathering rates' –> ' overestimated rates of weathering' L19.So it was not tested? Is this future research? It seems to be more of 'ongoing' research? L20. Is this catchment scale weathering? 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. L28 to L4 Page 24. Not future... ongoing /current research? Page 24 L5. See comments above... much of the text presented so far under 'future research' speaks more to limitations in application or uncertainties. L6. This is PROFILE specific text... not the depletion method, or others... L8. Often? L9. 'Not only are...' L13 to L20. This is a very uncertain uncertainty... why so much space for something that is not 'very uncertain'? L24. Unless that span is used to estimate the

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uncertainty... L26. This does not make sense. Is it possible to contain the solution space? L28. This is not true. However, many users make that assumption. However, others do not. Page 25. L1. BET may still be the best technology? L2. Are the 'current uncertainties (?)' quantified? Are there uncertainties? L5 to L9. Repetitious text. L10. Improved soil moisture should come with improved soil hydrological modelling... L12. All estimates are modelled? What are the other? 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' L23. Manual? This is a bit trivial... delete and move to personal 'to do' list. I suggest you write a paper on this. L28. They were not outliers. They represent measurements for different compartments. This is well understood. 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). L16. Good but you can more clearly call it out as an absurd suggestion. L22. Was some of this difference on single sites driven by differences in depths / inputs? 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). Page 27. L10. Other approaches?

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