

Interactive comment on “Current, steady-state and historical weathering rates of base cations at two forest sites in northern and southern Sweden: A comparison of three methods” by Sophie Casetou-Gustafson et al.

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Answer to referee 2:

We would like to thank the referee for helpful comments. (1) We agree with the referee and think it would be possible to give more detailed method description. - Physical erosion: The effect of soil erosion is negligible at our sites due to relatively flat terrain, dense ground vegetation cover, stable till soil parent material and young soil age. The soil surface has been covered by dwarf-shrubs, lichen and mosses for very long time,

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resulting in low erosion. A general evidence for long time vegetation cover is the rapid vegetation establishment in areas where glaciers are retarding. (2) -Equation for calculation of weathered loss of mobile elements: We agree with the referee and will include an equation developed by Olsson and Melkerud (1989), which is based on the same principles as the mass transfer function. -Volume change factors: Fractional volume change (Vp) according to e.g. White et al. (1996) was calculated in Stendahl et al. (2013) for similar soil types (not presented in paper) showing positive values down to 40 cm depth indicating volumetric expansion, and further below it was 0 indicating no volume change. Data on Vp could be added to the current manuscript as well. -Table 2: By mistake we reported Zr in the wrong unit as well as showing too many decimal places after the comma. Therefore, we agree with the referee, we should report Zr in ppm. -Assumption about homogeneous parent material: The BC/Zr-ratio (Figure S3) was used to help defining the reference soil layer as it shows heterogeneities in parent material with depth. In case of heterogeneities in the profile the reference layer was chosen above this heterogeneity. -In response to the explanations given by the referee about depth gradients (i.e. “However this is the age of the soil at the surface, and soils at deeper depths should be much younger. Indeed, as weathering proceeds, soil thickness increases (≈70 cm according to this manuscript in southern and northern Sweden). If so, the age of soils decreases with depth. At the soil/parent boundary, the soil age should be zero. If so, how can release rates of cations be quantified using one age? This probably also explains partially why weathering rates are much higher at surface than at depth”), we argue: Our ultimate purpose is to estimate the long term average weathering per m² per year. In our calculations of mean annual depletion since deglaciation, ‘age’ is simply a constant number of years at each site. The degree of change in a soil layer should then be referred to as ‘loss’ or ‘change’, not ‘age’. Of course this means that we (and many others using the depletion method) assume that weathering has been more or less constant over time, which is not quite true since there is a gradual decline in weathering rate due to loss of minerals, formation of coatings etc. However, in these very young soils the assumption is probably more accurate.

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(3) -Steady-state description: Steady-state means that the state is dependent on the ecosystem properties and mechanisms that prevail under current climatic and biotic influence (i.e. PROFILE simulates present day weathering rates under steady conditions, which means that all parameters are kept on a constant level). The PROFILE model was developed in the context of determining critical loads of acidity, where the major interest were long-term sources and sinks of acidity (i.e. processes that are irreversible over the time scale of decades). As such, Sverdrup et al. (1990) justifies the use of long-term kinetics (i.e. steady-state kinetics) for mineral dissolution for young soils (such as Swedish soils). -Parameter table: It is possible to describe the input parameters in a simplified way in a table (i.e. not per soil layer, but per site or alternatively showing ranges). An example of an input parameter table is given in Akselsson et al. (2004, 2016). - Reporting data used in sensitivity analysis: We have reported these data in the Supplementary material (Table S1 and S2) - Residence time of soil water: PROFILE is not a dynamic model. Parameters are not modeled as a function of time. The weathering rate for a specific soil layer is dependent on the degree of soil moisture saturation. Weathering reactions can only take place on wetted surfaces and PROFILE assumes that all exposed mineral surfaces are wetted. Precipitation input data are used to describe the vertical water flux through the soil profile. The flux leaving a soil layer is calculated based on annual average precipitation and runoff in relation to the relative base cation uptake. Both the water flux and the soil moisture content are fundamentally involved in calculating soil solution equilibria for each soil layer based on steady state mass balances (Equation 3.2 to 3.6 in Warfvinge and Sverdrup, 1995).

(4) The referee asked: "How much error is this in the overall BBC"? We answer: The revised version will include estimated uncertainty ranges (equivalent to 'confidence intervals') for the base cation budgets and weathering derived from them, based on information on between-plot variability and, where this is not available from the present experiments, on estimated uncertainties calculated by Simonsson et al. (2015; Table 4-8 in that paper); the field trial of that paper has large similarities to the present studies regarding the study design.

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- Total atmospheric deposition: We agree with the referee. It is not necessary to refer to the canopy budget model, since canopy exchange is not considered by PROFILE. However, by calculation of dry deposition factor we correct for canopy exchange. In order to distinguish canopy exchange from dry deposition for Ca, Mg and K, Na was used as a tracer ion. Dry deposition for Na and Cl was calculated as the difference between wet and throughfall deposition. Wet deposition for all elements was calculated as outlined by Zetterberg et al. (2014), i.e. correcting bulk deposition for dry deposition using wet-only to bulk deposition ratio. Finally, total deposition for all elements was estimated by summing up dry deposition and wet deposition. -With regard to the leaching, the referee wrote: "Leaching (LBC) is estimated using three soil water samples collected at 50 cm. Is this for one year only, or multiple years? This is not an optimum method as soil water chemistry is known to vary significantly over seasons (e.g., a function of soil water residence time, biological uptake etc). Can you explain how different water chemistry is and when exactly the samples were collected?" And we argue: Soil water sampling was performed twice every year, i.e. in the spring and in the autumn, which is the period of highest water flux and which means that the most important leaching events are covered. An additional illustration of the variation in water chemistry with time of the year could be given as supplementary figure to show BC fluxes for this period and to compare with the drainage flux: Mean and Standard error of BC ions (mg/l) in soil water sampled at 50 cm depth in the soil of the four control plots at Asa during 1990 – 2004. The samples were collected twice every year, in the spring and in the fall. The spring samples were collected soon after the snowmelt and depending on current weather a specific year this meant that the yearly spring sampling date varied between the last week of April and the last week of May. The fall samples were collected late, i.e. soon before frost in the soil was expected. That meant that the fall sampling dates varied from year to year, i.e. from the first week in September to mid-November. - Evapotranspiration: We agree with the referee, it is entirely model-based. However, there are ways to validate it, i.e. Alternative ways to estimate water balance can be used. Calculated drainage compare well with nearby catchment runoff and drainage

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and evapotranspiration compare well with Swedish Meteorological and Hydrological Institute (SMHI) results from our study regions. -The referee asked: "Does plagioclase or amphibole weather to form smectite or other clay minerals and thus retain the cations"? We answer: Formation of smectite, or any other clay mineral with a CEC, potentially increases the pool of exchangeable cations in the soil. Small amounts of smectites are known to form in podzolized soils (Olsson and Melkerud, 1989). However, at the time scale of the present cation budget, which involved a fraction of a tree rotation (a few decades), we can expect changes associated with cation accumulation in the biomass to vastly outnumber any changes in CEC due to any clay formation. To the extent it occurs, any increase in exchangeable cations due to clay formation is included in the base cation budget; therefore, it does not represent a methodological problem. By contrast, release and fixation of non-exchangeable cations in clay minerals, e.g., of interlayer K (which may be significant even in soils poor in clay minerals; Simonsson et al., 2016), contribute positively or negatively, respectively, to the unknown sources and sinks summarized as the 'weathering'. - Discussion Line 521-529: We agree with the referee that this section can be improved and we suggest that we merge 4.2 and 4.2.1 and keep the title from 4.2.1 and then we decide what parts of 4.2 are still relevant. We should also introduce a sentence directly in the beginning of 4.2 "Our first test criterion was to see if. ...". The test criterion can be estimated from the magnitude of weathering but also from the proportions of the estimates of depletion versus PROFILE." When we speak about the magnitude of weathering, maybe we should focus on soils of similar pedogenesis (Swedish studies) and where the same soil depth has been considered for weathering calculations. We should then write about high similarities or/and mention studies where there are dissimilarities. -Discussion Line 542-555: The discussion about different $W_{\text{profile}}/W_{\text{depletion}}$ ratios allows us to estimate our first test criterion from the proportions of the estimates of the depletion method versus PROFILE. The use of ratios is also a way of comparing our results to other studies even though the study design might differ (i.e. sampling depth etc.). -Discussion Line 586-587: The bulk density influences the exposed mineral surface area that is used by PROFILE.

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This is one important model parameter (see sensitivity analysis of PROFILE by Jönsön et al., 1995; Hodson et al., 1996). It is unclear how this is an artefact. - Discussion Line 598-610: We think that this section adds relevant information to the interpretation of the results of our manuscript, despite that the inhomogeneity/mixing problem is general for all elements, not only Zr. The mixing erases the depletion gradient that we utilize to quantify element loss. This is an important issue to discuss since the depletion method is widely applied. Whitfield et al. (2011) made an effort to describe Zr variation with depth and highlighted the fact that this is unfortunately rarely done. Contrary to our study, Whitfield et al. (2011) included at least 10 soil profiles where the Zr gradient showed opposite trends, i.e. overall enrichment of Zr in the rooting zone. They related these disturbances to the occurrence of glaciofluvial deposits and aeolian till in their study region. In line with Whitfield et al. (2011), we think it is important to highlight which profiles are unsuited for use. In our study we had 7 profiles that fulfilled the assumption of Zr enrichment towards the surface and in one case we used the Ti concentrations since we argued that Zr showed a disturbed gradient. -Line 611-620: We suggest to remove this part, since it is rather speculative. - Figure 3: We agree with the referee and will add a description about how error bars were derived. Minor comments: - By acid silicate bedrock we mean felsic (granitic) bedrock. - Line 94: The referee wrote "recovery is driven mainly by silicate rocks? What about carbonate rocks?" and we answer: We did not detect any calcite in our mineralogical analyses of the soil samples by XRD. Carbonate weathering could be expected in the Caledonian mountain range or from weathering of carbonate-bearing till close to the Caledonian mountain range and confined areas with sedimentary bedrock (e.g. Gotland and Scania). The sites of this studies lie outside the influence of these areas. The bedrock of the Flakaliden site belongs to the Svecokarelian province and the bedrock of the Asa site belongs to the Transscandinavian Granite-porphry belt (Fréden, 2009). The study by White et al. (1996) highlighted the importance of small amounts of calcite in intact granitoid rocks and its significance for Ca found in watershed studies. The mean value from the above-mentioned study by White is 0.25 wt. %, and the median 0.075

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wt. % calcite. White also noted that in laboratory leaching experiments on the rocks they studied reactive calcite became exhausted after just 1.5 yr. Given the trace concentrations involved and the high solubility of calcite we doubt very much that calcite is or has been of any long-lived significance in the soil profiles studied, even though they are derived largely from rocks of granitic composition. Though we agree that Whites results indicate that calcite in the in-situ granitoid bedrock underlying the soils probably will contribute to Ca export from the catchment. - Line 181-183: We will correct for these spelling mistakes. - Line 197-Line 200: Yes, the solid fertilizer mix contained Ca, Mg and K (see table 2 in Linder, 1995). NB! The mass balance estimates of weathering were only based on measurements in the 4 control (no fertilizers) plots. - We agree with the referee and will correct for the following mistakes that the referee has pointed out: "Line 216: Use either 143 C5000 and 10 150 years, or 143 and 10 thousand years. Line 255: space between "and" and "above". Line 267: Figure 5 is cited before Figures 2-4? Can you renumber them? Line 425: Misspelling: Weatherable? Avoid paragraphs with just one sentence, such as those in Line 487-Line 489."

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