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

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Referee 2 starts off with a paragraph with general comments, describing the paper and its strengths. In this paragraph there are no questions or suggestions of changes. After the general comments, a few specific comments are listed. Below we list the specific 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.

Specific comments 1a. The consistency of the weathering rates estimated by different methods at the same sites in this study was remarkable. Older studies frequently reported one or two order of magnitude differences in estimates of weathering by depletion versus budgets. . . . . Have weathering rates changed substantially in the last 30 years, or were the differences always muted in Swedish soils? In our site level comparisons we have included both old and new estimates of weathering rates, for all

sites in Sweden that we have found where estimations have been done to the same depth with at least two methods. We think that the consistency is remarkable on some sites (e.g Gårdsjön C taken from a publication from 1998) whereas there is less consistency on other sites, e.g. Asa, Flakaliden and many of the sites from Stendahl et al. (2013). The differences are however not one or two orders of magnitudes. If the studies that Referee 2 refers to are from Nordic sites, we are not aware of which they are. We know that such large differences have been seen in other settings though. One example is when mass balance estimates of weathering are included. We have commented on this. Both the uncertainty in the components of the mass balance, and the possibility of changing pools render this an unsatisfactory method for weathering estimates. In our case several mass balance estimates are included, and they do expand the range of estimates, even though not to order of magnitude levels. Another way to create large uncertainties is to compare studies that have some fundamentally inconsistent assumptions, such as depth of soil considered. This is the situation at Svartberget, where the differences were orders of magnitudes as reported by Klaminder et al. (2011). In that study weathering rates for different root depths and from catchment level calculations were compared, and site level estimates were compared with regional estimates. All of the differences between those estimates cannot be attributed to uncertainties. We only included the estimates from that site, here called Svartberget B, that referred to the same exact site and the same root depth. There, the budget calculations gave weathering rates more than twice as high as the depletion method, which is the case for three of the four sites where both depletion method and the budget method has been applied (Table 1): Svartberget B, Flakaliden and Asa. Only in Gårdsjön the results are of similar size. We have not done any changes based on this comment, we would need additional information about which studies Referee 2 refers to.

1b. Following upon the previous comment, have the authors tried plotting the estimated weathering rates against one another? For example, PROFILE vs. Depletion, etc.? Perhaps this is in one of the other papers in the series In Stendahl et al.

(2013) weathering rates from the depletion method was plotted against PROFILE weathering rates on 16 sites. The number of sites in that study is enough to be able to draw general conclusions about differences in weathering rates from the two different methods. In the present study, those sites are included, but also other sites, based on different combinations of methods. Based on the material in this paper, it is not as straight forward to do complementary pairwise comparisons, since most of the approaches (all except PROFILE and the depletion method that are already compared) are performed only on a few sites. Instead we have focused on presenting the weathering rate intervals for each site, as a way of framing the weathering rates. 3. “When comparing weathering rates to harvesting removals, what rotation length was assumed to calculate a meq/m2/yr value?” We use site quality (biomass per area unit and year) to calculate harvesting removals, in the same way as in Akselsson et al. (2007: 2016) and Stendahl et al. (2013), which we refer to. Site quality (in Swedish “bonitet”) is the biomass growth on a specific place if the forest is managed optimally. In our calculations we assume that the actual growth is 80% of the optimal growth. The average growth in the site quality concept “assumes “ that the forest is harvested at the optimal point in time, which varies between north and south and between stands, from about 70 years in the south to 120 years in the north. We will explain more thoroughly how the harvesting losses were estimated. 4. “An awful lot of conïňĄdence is placed in this paper in modeling approaches in general and in the PROFILE/ForSAFE family of models in particular They are good models conceptually and they produce results that appear to track the results from other methods (but see comment 1b above). The problem is that there are no "measured" values of weathering ïňĆux to use to validate these (or any) weathering models. So, just as budget-based approaches may be contaminated by non-weathering ïňĆuxes like net losses from exchange sites, and depletion approaches may suffer from invalid assumptions, model results almost certainly contain a host of errors. This is addressed somewhat in the paper. Depletion methods and budget methods are based on ïňĄeld observations and data. With all their ïňĆaws, they are, at least in my view, fundamentally stronger than model

results. To compare them as equivalent approaches is problematic.” We agree that the depletion method and the budget method, that are based on measurements, are very important in framing weathering rates, which we also point out in the paper. However, we don′t think that the results from those methods are fundamentally stronger than the model results (which actually also are based on measurements to a large extent, e.g. laboratory measurements). On the contrary, we think that further studies are required if more robust results from those methods are desired – something we point to in the conclusions. There are not short-cuts to certainty, but in this paper we have tried to show how far we can go with the available publications. Regarding budget calculations our results show that the most extreme outliers came from the budget method. We explain that by the fact that other sources than weathering are included as discussed in Rosenstock et al., in review (this issue), as well as the documented uncertainty in terms used in the mass balances. We conclude that “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.” The depletion method gave unrealistically low weathering rates in some cases. On one of the sites, Asa, that was studied in detail within another paper in this special issue (Casetou Gustafson et al., in review), the analysis of the soil profile indicated that the soil profile has been disturbed, introducing errors in the weathering estimates. We highlight the importance of excluding sites with disturbed profiles as well as to perform studies where the average weathering rate since the last glaciation is related to the present weathering rates are required, to be able to make necessary adjustments of the historical rates.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2019-1/bg-2019-1-AC2-supplement.pdf

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