Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-47-RC1, 2019

Biogeosciences
Discussions
Open Access

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.

Anonymous Referee #1

Received and published: 28 February 2019

This paper compares three methods (elemental depletion, PROFILE and catchment budget) for estimating base cation weathering rates at two forested sites in Sweden. I understand that this is part of a special issue, but in my opinion, this paper offers nothing new to the literature except that these established methods have been applied to two new sites. Given the lack of any originality, limited sampling and an incomplete consideration of the large uncertainties associated with all these methods I do not feel that it should not be published. There must be at least 10 previously published papers (some of which are cited; e.g. Klaminder, Futter, Bain, Whitfield etc.) that have done

the same thing as presented here and in general they all show the same thing – base cation catchment budgets are typically the largest (as some base cations are lost from the exchange pool), followed by PROFILE with the elemental depletion method usually being the lowest. The fact is that all methods have large uncertainty, and this is not adequately captured in this paper. For example, we know for sure that the budget model cannot adequately capture weathering as there are always losses/gains from the exchangeable pool and soils are heterogeneous. The concluding sentence in the abstract "The large discrepancy in weathering rates for Ca, Mg and K between mass balance and the other methods suggest that there were additional sources for tree uptake in the soil besides weathering and measured depletion in exchangeable base cation" is completely unfounded and should be removed. The inaccuracies in all the methods are such that the discrepancy is most likely because of uncertain weathering rates combined with unknown variability associated with changes in soil exchangeable pools and uptake. Further, in the abstract mean numbers are presented, but each of these has a large error associated with it so what is the point? The study design itself is such that the data are extremely uncertain – four plots at each site and a single sample from each plot? [4 samples per site]. This is insufficient in my opinion. As stated – the comparison between PROFILE and elemental depletion has been done probably at least 10 times before.

The introduction and methods that describes these methods in detail is essentially a repeat of other studies – I have read this before. The PROFILE uncertainty appears to be part of another paper in this special issue which further questions why this paper should be published. The authors show that surface area affects weathering estimates a lot in PROFILE (which we know), but it does not seem that the authors actually use mineral surface area but use bulk soil surface area, which is not the same thing. Other inputs to PFOFILE are estimated and ultimately lead to considerable uncertainty in the estimates.

The mass balance is uncertain because 1) soil is so variable meaningful changes cannot be determined accurately [probably the most uncertain part of this budget], 2) forest growth was assessed at the plot level (not at the pit level) and tree chemistry in addition to biomass is very uncertain/variable, 3) export is essentially modelled [runoff] and sampling frequency is limited. Simply judging the parameters as high or moderate quality (lines 405-407) is a subjective aspect that should be included in a scientific manuscript. Given the fact that really this paper provides no knowledge beyond describing the data that the authors have for these two sites there is little point commenting on the paper in general. It is far too long, compares mean values estimated by different weathering rates [which is pointless for reasons mentioned above and also because in some case samples were “excluded”]. I don’t believe that this paper does anything that previous papers did so while they have used approaches that others have used – they of course get the same results, but at two different sites. I don’t think that this is sufficient to warrant publication, but I expect that as it is part of a special issue it may be decided to keep it. Given the lack of novelty and my opinion that it should not be published I do not wish to make a lot of editorial comments as my job is to decide whether the paper is a novel contribution or has major flaws (see above), not to edit a paper that I do not think meets these criteria.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-47, 2019.