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Discussions

Interactive comment on "Si cycling in a forest biogeosystem – the importance of anthropogenic perturbation and induced transient state of biogenic Si pools" *by* M. Sommer et al.

Anonymous Referee #2

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General Comments

The manuscript submitted by Sommer et al. presents original data on the biogeochemical cycle of Si in a temperate forest from Germany. Recent publications have evidenced land use impacts on the Si –cycle but the full understanding and the extent of those impacts remains poorly constrained. The present work is a contribution to the understanding of the parameters which controlled the output of DSi from a forest developed on acid soil almost composed of quartz, a highly resistant mineral to weathering. The manuscript provides very valuable information concerning the annual variations of Si concentration in leaves and the quantification of the zoogenic Si pools. The main

criticism is that the conclusions are not enough supported by the data. Indeed, the quantification of the soil BSi (phytoliths) using two different methods are not consistent and the description of the phytoliths does not follow the international standards. The authors should have given the counts of the shapes (ex; elongated; short cell) and not the counts in term of plant types. I recommend major revisions.

Technical comments

-p. 18873/27: replace "method manual of Alexandre" by Alexandre et al., 1997. -p. 18878/ 24 : give ranges or averages $\pm$ SD for Si values -p. 18879/2: ..."the size of biogenic Si poll in different horizons": adsorbed Si can also explain the Si extracted by water; please comment. -p. 18879/5: give ranges or averages $\pm$ SD ; how do you explained that water extractable Si and Tiron Si have the same distribution in the soil profil (decrease down to 100-150 cm) ? -p. 18879/18-20: the conclusion came too early in the text without a comprehensive evaluation of all the hypotheses ; for instance , 1) there is no evaluation of the validity of the values of Si extracted by Tiron. Because Tiron is known to dissolved crystalline silicate it can over estimate the measurement of the biogenic Si pools ; 2) the extraction by heavy liquid flotation can lead to loss during the procedures but without replicates, this hypothesis cannot be assessed; 3)the loss during the removal of the < 2 fraction has already been discussed (ex ; Saconne et al., 2007) but is not fully assessed here; assuming that all their BSi is concentrated in this fraction it should have been detected using various techniques (XRD, IR, chemical extraction. . .) ; I also recommend to separate the discussion from the results. -p.18880/1... and fig 2: the authors should present the shapes of phytoliths before assigning them to plant type. -p.18880/25: you mean "compared to Si (phytolith) given by physical extraction but 3 orders of magnitude less that Si (phytolith) given by Tiron extraction"? -p. 18881/ 5: no data from dust (solid) input ? -p. 18884/13: you mean (high) Si concentration in the waters (6 mg/L) or in the plants ? because in both cases the concentration are low compared to other systems -p. 18884/21: this statement is not robust if we consider the kinetics ; at acid pH the rate of quartz dissolution is an

order of magnitude less than the one for albite for instance so the Si release from 3 % Albite should be equivalent to the Si release from 30 % quartz. -p. 18884/25 : the composition of the waters are not enough constrained to support the conclusion. The recent study of White et al (2012, GCA) shows that at the level of the weathering front the concentration of DSi can be controlled by dissolution of primary minerals whereas at the surface, DSi is controlled by phytolith. Here, the concentration of other elements such as Na, Cl Al would have been useful to constrain the weathering processes. -p.18886/17: "high DSi concentrations" compared to what ? -p.18886/22: not enough constrained by the data unless the description of the phytoliths is more rigorously done and the pools of BSi are more clearly evaluated ;the change of the vegetation 20 years ago may have changed the weathering rates and the biogeochemical cycle but no historical data are presently available to support the conclusion for the moment ; the model should have been discussed with the recent papers presenting evidenced for anthropogenic impact (Conley et al., 2008; Stuyff et al., Casey and Fulweiler ; Guntzer et al.)

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