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Interactive comment on "Silicon cycle in a temperate forest ecosystem: role of fine roots and litterfall recycling and influence of soil types" *by* Marie-Pierre Turpault et al.

Anonymous Referee #2

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In this manuscript, Marie-Pierre Turpault et al. address the role of fine roots, litterfall and soil type on Si cycling in a temperate forest system. The main and surprising novelty of this manuscript lies in the observation that fine roots actually are a large Si reservoir in forest soils. To my knowledge, no other authors have ever performed a similarly detailed exercise to quantify the amount of Si in the forest root system. Quantifying root biomass is difficult, and these authors have done a tremendous effort to take on this challenge.

While this is a finding worth publishing in itself, I have strong reservations regarding the mass balance the authors have made for the whole forest ecosystem. These reserva-

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tions are mainly related to the applied methodology to analyse for Si in the soil system, which is inadequate to assess the complicated Si cycle in the soil, as it does not distinguish any pedogenic nor biogenic Si fractions from the abundant mineral fractions. This prevents to make any major conclusions on the role of soil type in the Si mass balance, and also makes it difficult to assess the cycling of litterfall Si in soils, once dissolved. Multiple secondary pedogenic fractions are accumulated deeper in the soil.

In conclusion, I am impressed with the root Si quantification the authors have performed, and I think that a focused manuscript emphasizing the importance of roots in the forest Si cycle is worthy of publication. I also think that a more focused manuscript would have a larger impact on the interested scientific community. The authors should either improve methodology if they want to address the full Si cycle in the forest, or far better emphasize the methodological shortfalls in their discussion, that prevent to make any statement on the full forest Si cycle, and focus on the interesting story of the roots. I will make more detailed comments below.

Line 45: I am becoming a bit annoyed by all Si manuscripts starting with the same statement. Can we just accept that it is now common knowledge that there is a lot of Si in the Earth's crust, and that minerals dissolve. This manuscript is about forest Si cycling, and the role of biological processes in the Si cycle. This has been well described in several review papers over the last years (e.g. Conley, GBC, 2002, Volume 16; Cornelis et al., Biogeosciences, 2011, Volume 8; Struyf Conley, 2012, Biogeochemistry, Volume 107).

Line 57 and beyond: I really don't see why this is important to this manuscript. The division between accumulators, excluders and neutrals is anyway arbitrary, if based on concentration. The Si uptake of plants is also governed by external Si factors, such as its availability.

Line 62: Why also? You have not referred to forests before, so 'also' seems out of place here. What about wetlands, one of the most studied system in the biological Si

cycle? If you provide a list, wetlands should be there.

Line 67: Here comes the first reference to later methodological issues. This statement is untrue. In recent years, methodologies have been developed that allow to distinguish pedogenic, reactive mineral and biogenic Si phases in soils (e.g. Barao et al. 2014, European Journal of Soil Science, 65, Barao et al., LO Methods, 13, 2015; Georgiadis et al. , 2015, Soil Research 52).

Line 76: soap? Probably sap is meant.

Line 90: the second hypothesis is not really novel, Cornelis (et al.) (see also reference list of paper) has already published multiple papers on this issue. In these papers, he shows that methododology is quintessential in addressing the complicated soil type-Si cycling coupling, and the applied method that does not distinguish any secondary soil Si fractions from minerals is inadequate to address the hypothesis.

Line 93-95: awkward wording, consider revising

Line 104: Why? If you want to address the whole forest Si cycle, the soil is of the essence. If you do not apply best available methods (see above) here, then you start with a strong handicap.

Line 113: without any reference to these networks, their relevance is not clear.

General: ceramic cups? Why not plastic? Can ceramic cups potentially add Si to solution? Has this been tested?

Line 209: total fusion is unable to provide sufficiently detailed results for assessing soil Si cycling, where multiple secondary Si fractions form that are actually essential in the whole ecosystem Si balance.

General: I miss any comparison with recent studies that have also made forest Si efflux quantifications. How do your fluxes compare to e.g. Struyf et al. (2010, Nature Communications, 1 and Clymans et al. 2013, Biogeochemistry, 11). I think a section putting

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the observed effluxes in the context of other literature, would be far more interesting than the attempt to discuss the role of soil Si processes in the forest Si cycle, given the flawed methodology here. The suction cups do provide an idea of the leakage, and focus should be on how this compares to root turnover and forest Si uptake. In general, I have the impression that Si efflux in this paper is rather low compared to other studies. Is this maybe because these are young forests? Or due to management?

Line 451: Consumption during autumn? Rather contradictory to forest growth in spring and summer? Pedogenic processes at play? Also in apparent contrast to later references to a net Si efflux in fall (Line 536)?

Line 462: I would not use “global” in this local ecosystem context

Line 515-522: I don’t understand. First a significant accumulation is discussed, but a few lines below limited accumulation is mentioned?

Line 547: I don’t understand how you can state the biological origin, if you apply total fusion.

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