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Biogeosciences Discussions

Interactive comment on "Tracing the origin of dissolved silicon transferred from various soil-plant systems towards rivers: a review" *by* J.-T. Cornelis et al.

Anonymous Referee #1

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Up until a couple of decades ago, terrestrial processes involving silicon (Si) cycling were thought to be primarily physical with little role for vegetation. Hence Si was treated like sodium or chloride as a tracer of weathering and leaching that could be evaluated without concern for biocycling. That view is no longer tenable given the recognition of the importance of plant roots in sorbing Si and routing it into above ground tissues, which eventually leads to return to soil surface. Subsequent to this realization many questions have arisen related to plant sorption processes, to formation of the Si bodies (phytoliths and other materials), to stability of the returned Si compounds (opal A),

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and the interactions among these compounds, primary minerals, and secondary soil minerals within the soil. There has been a lot of exciting work published in the last 15 years that focused on these questions and in the process breathed new life into development of direct isotopic tracers as well as pseudo isotopic ones that provide a clearer understanding of the sources and fates of Si in the terrestrial biogeochemical system.

This paper compliments and updates recent reviews by Alexandre, by Conely, by Sommer, by Street-Perrott, and by Exley, as well as less detailed reviews at the beginning of most research papers, all of which deal with Si in the terrestrial weathering zone and its relationship to biological and hydrological processes. The focus of this review is strongly on the source and fate of dissolved Si (DSi) as it moves from soil to rivers (and/or implicitly to groundwater). It covers the literature broadly and presents the high points without getting involved in the details of the processes involved. This is perhaps a blessing when dealing with the evident complexities of the terrestrial system and the present state of knowledge with regard to interpretation of the emerging tracers. In the latter regard we are perhaps entering the stage where the promise of the tracers is giving way to a recognition that considerable complexity exists in their use. Except to note that tracers are of little use without mass balance and vice versa, and that the tracers are affected by a number of potential side reactions besides the major ones of interest (biocycling and hydrological mobilization) the paper avoids dealing with issues involving identification of fundamental fractionation (or discrimination) factors, multiple process controls that may force fractionations is the same direction, and additions of Si from external hydrological or atmospheric sources. Still this reviewer hungered for fewer general statements and more detail on the linkages among individual studies conducted in varying parts of the world, more synthesis based on the accumulated knowledge.

The paper does reach for such a synthesis in figure 3 where a summary of global process domains is presented and interpreted. The figure focuses on 1) weathering sys-

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tems where there is plenty of weatherable minerals, and favorable climate for mineral decomposition and Si leaching, 2) weathering systems where most primary minerals and indeed Si-bearing secondary phases have been exhausted but where climate is still conducive to leaching losses, 3) weathering systems where there are primary minerals available to release Si, but where climate and/or internal pedogenic processes are not conducive to substantial release and removal of Si, and 4) arid systems which seem to be implicitly defined as having no plants and no weathering – in other words hyperarid. The first two figures in the paper are modified from other reviews, whereas Figure 3 is a creative effort to develop a synthesis. Unfortunately the figure and its discussion are not as well fleshed out as is necessary to really make a significant contribution. What are the actual environmental bounds for the different domains and what quantitatively does it mean to have "optimal climate and soil conditions"? How do different rock types modify the expected outcome from each of the boxes (e.g. granite has much of its Si tied up in quartz whereas basalt has less Si but more is actually available)? What is the justification for breaking domain 3 into two subcomponents, one with a specific soil process defined? It seems like domain 3 (non-podzolic) is left to cover all grasslands and savannas, and desert shrub steppe – there seems to be an implicit break whereby Earth's dry lands are not important in the analysis because they do not contribute DSi to rivers, but if so, it needs to be presented and justified as a boundary condition to the analysis. The analysis of each domain and its DSi source and contribution to rivers is scattered and does not draw on the totality of the literature presented earlier in the paper. Several of the studies discussed earlier explicitly analyze chronosequences that cross between domains a and b, other studies can be lined up to effectively do the same (keeping rock type and vegetation similar, etc.). In a similar fashion, several studies have analyzed climosequences that cross between a or b and c, or cut across a climate gradient within c. Or again various studies can be linked to explicitly get at the trends occurring with changing climate. The reason for singling out the need for a deeper analysis of figure 3 is first that it is the most interesting part of the paper and second that in-depth studies of Si balances and contributions to DSi demonstrate that the control processes are not as intuitively obvious as what might be expected and indeed as they are drawn in figure 3.

In summary, this is a good review and useful particularly to the broader biogeoscience audience. It is publishable with minor editing. However, it may lead one to think that the subject is clearer than it is once individual studies are considered in detail. Considering the number of recent reviews on Si that are vying for attention, the authors might want to consider a somewhat more adventurous approach to the subject. I suggest a possible change in the balance such that figure 3 receives more careful focus and the review revolves around it rather than around figure 1 which is pretty standard fare.

Interactive comment on Biogeosciences Discuss., 7, 5873, 2010.