

Interactive comment on “How big is the influence of biogenic silicon pools on short-term changes of water soluble silicon in soils? Implications from a study of a ten-year-old plant-soil-system” by Daniel Puppe et al.

Anonymous Referee #2

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The authors present an interesting and detailed examination of the various constituents of the biogenic silicon pools within a constructed watershed in Germany. There is a detailed methodology outlined in this paper with some interesting results – both through time and through space.

My biggest concern is that this paper starts with a claim to uncover truths about biogenic Si pools other than phytogenic pools with in systems, but the work is mainly focused on a highly-disturbed, constructed watershed. Which is incredibly important to study, but I am not sure much of the introduction fits into what the study actually

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is. There is question of the broad, applicability of these findings which I only point out given the sweeping nature of much of the introduction. A recasting of this could solve this issue.

There needs to be more focus on the disturbance aspect—that is one way to take this work. In Lines 66-70 the idea of disturbance and disequilibrium is brought up and how that affects Si cycling, but not thoroughly explored. Further in the work, results are presented in a manner that there is explicit testing of various regions of the watershed and it is a bit confusing as to what is being tested as there are “initial values” that seem to be for the whole watershed that are considered to homogeneous (which the authors allude to) and then each section is independently tested against these initial values. Part of my confusion here stems I think from my misunderstanding of the closing section of the introduction as there is some obfuscation about what is being hypothesized.

At its core, this manuscript is a good survey of the biogenic Si pools in the Chicken Creek watershed. But currently, I feel the introduction and discussion read as if they are from two separate papers. I believe the introduction needs to be reshaped to fit the paper that is here. The methods section is excellent and there is a thorough write-up of the procedures presented with adequate documentation. This should be lauded as many papers are often lacking in such detail for those who would like to replicate experiments. Some of the background for this paper that is necessary is in the Puppe et al. 2016 paper in *Geoderma*—but this manuscript submitted here reads as a good companion piece to the *Geoderma* one.

There is an appeal here to many readers of Biogeosciences mainly in that Si cycling is not well understood or appreciated broadly in the biogeochemical community and this work has the potential to make inroads towards expanding the understanding of Si cycling and its relevance. There is a lot of potential here with a need to make the paper more uniform and clear. Presently, there it is too disjointed.

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Line INTRODUCTION 50 – I would consider changing the line “pro- and eukaryotic organisms...” to “prokaryotes and eukaryotes.” 59 – The phrase “big scale” reads to colloquially and I would suggest changing that. 60-61 – I would clarify this sentence a bit and tone it down the claim. Though this is a substantial amount, this one facet will not regulate all of the climate. 62 – Change “since” to “for” for clarity. 70-72 – I am interested here in how these are unbalanced. Could you expand more here? In what direction and magnitude, please. 73 – “. . .allow to analyze. . .” is not grammatically correct. 75-84 – I really like this summary of previous work and major findings here. 83-84 – Shorten “. . .as well as uptake into 83 biological systems” to just “biological uptake” 85-92 – There is too much effort needed to suss out what the hypotheses motivating the work are. They are in there, but need to be clarified. METHODS 107 – What do you mean by “serving as aquifer” here? This is worded strangely and could be interpreted in different ways. 112-115 – I think this area could be improved by considering the area of each portion of the watershed, and maybe even something like an upslope accumulated area calculation. There is some work here that depends heavily on hydrology, but there is not so much hydrology in here. Some GIS work could help. 116 – Wait, what is skeleton content? *Also, it is not necessary to put Chicken Creek in quotations each time. 157 – “weighted” should be “weighed” 160 – Change “was not used” to “avoided” 183-184 – This sentence is awkwardly constructed and could be clarified. 195-203 – The verb tense vacillates a bit and should be standardized throughout. 254 – Why were there two replicates before, and now three? Overall, good methods section. RESULTS 290-295 – I generally like this section, but you could present some percent change too as a normalized difference. This is usually a good way to focus what you want the reader to notice. 295-297 – This decrease in pH, this is interesting here. 301 – Usually you see it written $7.4 \times 10^{-3} \text{ g kg}^{-1}$ 323 -328 – This part gets really confusing when you say “increase to” and you present a range. I am not really sure where to follow with this. Could means with a standard deviation or error or some measure of uncertainty be more clear? * In general there is the presentation of results by different sections of the watershed, though this is not something presented as a hypothesis. If this is

the manner you want to present, maybe consider making this a research question and present a mechanism that could potentially describe the patterns. 365-367 – I am of the opinion that you cannot refer to a figure as self-evident of your results. The results section is the to describe the overview of these different pools. Our you could just cut this line. 372 – Do you have a sense for the total above-ground storage of Si in raw numbers? DISCUSSION 378 – This reads more like a topic sentence than a sub-heading * The discussion leads of with the origin of where the Si in the system is coming from. Obviously there is an importance imparted to this point, perhaps make this something you are testing then rather than just throwing out initially in the discussion. 393-397 – Great, here is the stuff about the sections being different. Maybe bring some of this up in the methods section where you describe the site. * Also, what is skeleton content? 428 – Larger, instead of bigger. FIGURES Fig 3- What are the error bars here? Without knowing it is difficult to believe that the South plot is statistically significantly different unless these are SD as much of the other paper, but with ANOVA wouldn't confidence intervals or standard error be a good alternative? Fig 4 – You highlight the different axes, but the differences are between A +B and C+D. Again, error bars. This really highlights the internal variance at the south site. What is going on there? You could really dig in there more in the future maybe. Fig 5 – A couple of notes here, technically this graph is pretty good. But given the large differences in total Si pool size, I don't think normalizing the scales is the best way to present this as it obscures relationships among the sites. It makes the t0 sites look much larger when we know they aren't. Also, thatching is often distracting when you could go full color for this journal. Again though, interesting stuff going on in the south section. Fig 6 – This is a really interesting way to present this as you have combined a table with a conceptual diagram similar to that of a textbook. I really like this. The font color differences are a bit distracting, but well done.

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