Reply to reviewer 1

Again, we would like to thank reviewer 1 for the comments on our manuscript. Please find our corresponding answers below.

In Introduction section, authors should give an objective summary to give a promising gap regarding phytogenic Si and soil properties affecting silicon mobility, which is quite important in the new findings and their significance of this MS. Please check all relative recent references. In addition, here I am not English speaker, but still find some grammatical errors, so that it will be better to improve its English for a better understanding before further publication. Please see the below soem specific comments:

The English of our revised manuscript was double-checked before resubmission. Furthermore, we reworked the introduction based on your insightful comments (please see our detailed answers below).

L. 15, this sentence should be rephrased since it is not clear to me 'most of these studies…. condition'

We rephrased the sentence and separated it into two:
Most studies are deliberately designed on the plot scale to ensure low heterogeneity in soils and plant composition, hence similar environmental conditions. Due to the immanent spatial soil variability, the transferability of results to larger areas, such as catchments, is therefore limited.

L.25, referring 'i.e., i.e., comparable to or markedly exceeding reported values for the Si storage in aboveground vegetation of various terrestrial ecosystems.' prefer authors to give experimental or analytical values/data'

We assume that you can follow our argument as stated in the first reply.

L.25, add ','after 'from our results…'

We added a comma here.

L.50, here, prefer to author should also refer that 'since soil properties affect soil silicon bioavailability, leading to the change in plant silicon content (see., Li et al., 2019., Plant and Soil 438 (1), 187-203 and others). In fact, any change in soil properties would largely affect silicon mobility and its accumulation in plants. It has been highlighted by recent studies, offering some nice evidences on this MS.

We added some (recent) literature to underline the importance of soil properties for Si bioavailability, and thus for Si uptake by plants and corresponding consequences for plant performance.

Line 37-38: Please cite relevant references to support 'in most terrestrial ecosystems phytogenic Si…' (e.g., Alexandre et al. 1997. Geochimica et Cosmochimica Acta 61, 677-682;

Blecker et al. 2006., Global Biogeochemical Cycles, 20; Cornelis et al. 2010. Biogeochemistry, 97, 231-245.Yang et al. 2020., Geoderma, 361: 114036). In particular, once being returned into soil, this phytogenic Si is largely are competitive with pedogenic silica, boosting the biological recycling of Si (Li et al., 2020., Geoderma, 368, p.114308).

We added references for our statement and also highlight the importance of recycling of phytogenic Si for Si uptake by plants.

L41-42: Other recent studies also reported that the grasses of the family Poaceae are generally Si accumulators.

We added a more recent reference to support this point.

Line 72-78: is it important or necessary for this MS to introduce these studies?

We rephrased and shortened the paragraph according your justified question.

Line 123-124 and Line 127-128: When the aboveground biomass of *C. epigejos* and *P. australis* were sampled? Is it in 2014? Please specify.

Aboveground biomass have been sampled a couple of days after image acquisition. We added this information to the beginning of the paragraph.

L256-257, L265, L272, L320-321, L 324-325, L334-335, L355-356, 379-380, Line: Use italics when showing the name of the species. Please check throughout the manuscript.

We corrected this mistake throughout the manuscript.

L324 (Figure 6): Please change the title of *y*-axes to “fresh biomass (green shoot)” in Figure 6a, and change the title of *y*-axes to “fresh biomass (green shoot + litter)” in Figure 6b.

Titles of y-axes were changed according your recommendation in Fig. 6.

Li334 (Figure 7): Please change the title of *y*-axes to “dry biomass (green shoot)” in Figure 7a, and change the title of *y*-axes to “dry biomass (green shoot + litter)” in Figure 7b.

Titles of y-axes were changed according your recommendation and, inspired by your proposal, we changed the titles of x-axes.

L372-403: a bit confusing about this section. Right now, the relationship between Si stocks of *C. epigejos*, *P. australis* and site properties was dubious just by comparing the variation trends between Si stocks and examined soil properties in different zones (e.g., Line 388-389: Among the examined soil properties, means of clay content (Fig. 10a) show a corresponding trend with respect to Si accumulation in dry biomass of *C. epigejos* for all three zones.). Could you perform statistical analyses between Si stocks (*C. epigejos*, and *P. australis*, respectively) and different site properties to show their relationship? At least Pearson correlation analysis is needed.

We agree and added results of correlation analyses to the revised version of our manuscript.

Line 379 (Figure 9) and Line 391 (Figure 10): What does the data on the top of box represent? Mean or median? What does the bottom and top bars represent? Please specify.

We changed the captions of figures 9 and 10 to explain the respective features.

Line 379 (Figure 9) and Line 391 (Figure 10): Right now, the readers do not know whether there are significant differences between zones. Could you perform significance test between the zones to show the significant differences?

We added letters (a and b; see modified figures below) indicating significant differences according to Kruskal-Wallis ANOVA ($p < 0.05$), changed the caption of figures 9 and figure 10. We amended respective sentences at the end of the paragraph starting at Line 387.

Line 401: Could you offer the data of soil moisture to support this conclusion: "As stated before, the occurrence of *P. australis* is governed by soil moisture conditions".

We assume that you accepted our explanation for this general statement and remained this conclusion unchanged.

Line 432-434: also recommend some latest literatures (straw remove, return, land use and management change) to support this point. e.g., "Li and Delvaux 2019. *GCB Bioenergy* 11, 1264–1283" and "Yang et al. 2020. *Plant and Soil*, 454:343–358".

Thanks, we added these important and more recent studies here.

L442-443: I confusion whether the climatic factors could govern the composition and structure of plant communities at Chicken Creek. I think the differences of climatic conditions may be negligible at such small catchment.

We rephrased the whole paragraph in order to clarify this point (please refer to the first reply)

L506-515: In my side, the current Conclusion is more like Discussion or Outlook. Prefer to move this paragraph to the end of Discussion section.

Thanks for this suggestion again. We followed your argumentation and moved the paragraph to the end of the discussion section.

L505: recommend the authors reconsider the Conclusions section by combining the main findings and significance of this study or answering the three major research questions raised in Introduction section.

Thanks again. We reworked the conclusions in our revised manuscript to better illustrate the significance of our results.

Reply to reviewer 2

Again, we would like to thank reviewer 2 for the comments on our manuscript. Please find our corresponding answers below.

Line 135, the authors should confirm that both “4.000 rpm” and “10.000 rpm” are corrected. Based on my experience, they should be “4000 rpm” and “10000 rpm” in this context.

We corrected this mistake.

Line 144, “Soil samples were analysed on various physicochemical soil properties”. The second “soil” should be deleted.

We deleted the second “soil”, which is, of course, an unneeded repetition.

Figure 3, the numbered signatures were confused. Why the small numbers (i.e., 01-07) were shown in Figure 3b while the big numbers (i.e., 08-16) present in Figure 3a?

We switched the figures according the logical order of class numbers.

Table 2. Is it more suitable to show the data as box chart rather than table?

We tried out a diagram for these data, but kept the table, because it appeared more intuitively accessible for the readers to our mind.

Table 3. The unit of RMSE should be corrected for C. epigejos, P. australis, and P.australis (litter incl. brown shoot). Specifically, “-2” should be labeled as superscript.

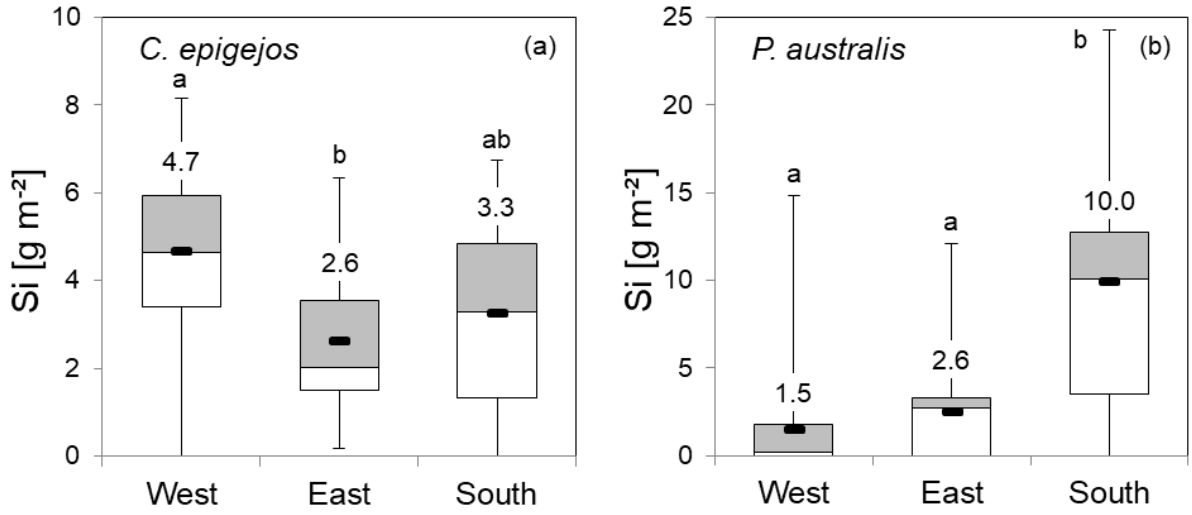
We corrected this mistake.

Figure 6. Why the R2 is lost for C. epigejos in Figure 6b?

Figure 6 was corrected according your hint.

Figure 9. Are there any statistical differences among West, East, and South for both C.epigejos and P. australis?

We performed a statistical analysis, added letters (a and b; see modified figures below) indicating significant differences according to Kruskal-Wallis ANOVA ($p < 0.05$), changed the caption of figure 9 and amended a respective sentence at the end of the paragraph (Line 387).


C. epigejos
(a)
a
b
ab
4.7
2.6
3.3
Si [g m-2]
0
2
4
6
8
10
West
East
South
P. australis
(b)
a
a
b
1.5
2.6
10.0
Si [g m-2]
0
5
10
15
20
25
West
East
South