

## ***Interactive comment on “Increasing soil carbon stocks in eight typical forests in China” by Jianxiao Zhu et al.***

### **Anonymous Referee #3**

Received and published: 28 September 2019

This manuscript examined SOC dynamics across China’s forests, using the direct measurements based on long-term resampling. The scientific question is important and the dataset is unique, and also the manuscript is well written. The following comments should be considered to further improve the manuscript.

Line 71: I think it is better to add one new paragraph to describe the characteristics of China’s forests (area, C stock, and the associated environmental change, etc.), and also the related research progress about SOC dynamics across China’s forests. The whole logic of the Introduction section will be improved by adding this paragraph.

Line 115: ‘in the two sampling periods’ should be written as ‘during the two sampling periods’. Same issues existed elsewhere.

[Printer-friendly version](#)

[Discussion paper](#)



Line 111-115: From this section, I understand that the sampling interval is largely different among various sites (also see Table 1). The original sampling was conducted during 1987-1998, and the re-sampling was performed during 2008-2014. It is interesting to establish the relationship between the rate of SOC change and sampling interval (or grouping SOC change by sampling interval) to examine its potential effects on SOC dynamics. In addition, I also notice that the sample size within each forest type is different among various sites. Is it possible to examine its potential effects on SOC dynamics?

Line 130-135: Was the same approach also used to determine both the bulk density and SOC content during the original sampling? If so, please clearly describe this point in the revised MS.

Line 156-158/165-167: Again, the sampling period varied substantially among various sites. Please add some descriptions to justify their limited influences on the subsequent data analyses.

Line 171-175: I think the authors need admit the potential uncertainties induced by the limited sample size (8 resampling sites) when upscaling these site-level observations to the national scale. Maybe you can discuss this issue as a potential limitation and also the future directions in the revised MS.

Line 193, and also in Table 2 and Figure 2: It is unclear why the authors focused on 0-20 cm, since 0-30 cm is more popularly used in the literature as the topsoil.

Line 209-211 and also Figure 3: I see that the largest increase was observed in sub-tropical forests, which had the deepest soils (0-100 cm). Did this pattern also hold true if you compare SOC dynamics within the same soil depth like 0-20 cm? It seems like not, as shown in Figure 2d. Please explained this issue a little bit in the revised MS.

Line 231-246 and also Figure 4: Given that climatic variables did not exert any significant effects on SOC changes ( $P > 0.05$ ), it might not be appropriate to incorporate

[Printer-friendly version](#)[Discussion paper](#)

them in the partial regression analysis. Please justify this issue in the revised MS.

Line 254-264 and also Figure 5: Please clearly describe how the authors consider the depth differences when conducting this kind of comparison.

Line 265-275: I am confused about the linkage between these arguments and any results observed in this study. Please clarify.

Line 281 and thereafter: It should be noted that, statistically, the relationships between SOC changes and climatic variables were not significant. To my understanding, it should not put too much efforts to explain those non-significant relationships.

Line 318-321: As mentioned above, uncertainties exist during upscaling. Please discuss this issue in the revised MS.

---

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2019-319>, 2019.

**BGD**

---

Interactive  
comment

Printer-friendly version

Discussion paper

