Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-83-RC1, 2018

Biogeosciences Discussions
Open Access
EGU

Interactive comment on "Patterns and controls of soil respiration and its temperature sensitivity in grassland ecosystems across China" *by* Jiguang Feng et al.

Anonymous Referee #1

Received and published: 2 April 2018

This manuscript made a contribution by compiling published data of soil respiration and temperature sensitivity related to soil respiration from five types of Chinese grasslands. The spatial extend of the dataset covers a large region. The temporal extend of the dataset is at the annual scale. It seems that the majority of the data points in this dataset have not been integrated into any published synthesis yet. Some aspects of the manuscript deserve attention. The authors carried out some basic correlation analyses on this dataset, and found some inconsistencies as compared with results in some published reports. One inconsistency was the correlation between annual soil respiration rate (Rs) and total soil nitrogen content (or total soil carbon content, because soil C and N tend to go together). As normally expected, most published reports showed

Printer-friendly version

Discussion paper

highly significant correlation between Rs and soil C &N, but not this manuscript. The actual causes of this inconsistency were unclear. Another inconsistency was that the manuscript did not find any significant correlations between climatic variable (e.g., temperature and precipitation) and Q10 values measured at 5cm or 10cm depth, which is in contrast to published results. Again, clear causes of this inconsistency were not offered. As the authors stated in the manuscript, the soil respiration in this context has two main components: autotrophic respiration of plant roots, and heterotrophic respiration of soil microbes. Therefore, the soil respiration should be controlled by both plant-related variables and soil-related variables. But unfortunately, there were only 7 data points that have autotrophic and heterotrophic respiration measured separately (and probably using questionable methods). Consequently, Rs and Q10 data could not be discussed in relations to plant-related variables and soil-related variables. Furthermore, these Q10 values were calculated using the seasonally changing temperature data which often highly co-vary with plant growth (therefore, the seasonal increase of root respiration). As a result, the seasonal increase of root respiration would contribute to abnormally high Q10 values. This key aspect definitely needs authors' attention. Changes in the Introduction, Materials and Methods, and Discussion sections are required accordingly.

The followings are minor editorial comments: Line 25: 'latitude and' should be removed here. These geographic features (e.g., latitude, longitude, altitude or elevation) may be used as proxies for temperature or precipitation in data analysis only when temperature or precipitation data were not available. So authors should consider eliminate all parts of the manuscript that use these geographic features in statistical analyses and any related discussion. Line 28: The % heterotrophic respiration was only based on 7 data points, therefore, should not be in the abstract. Similarly, if the authors really want to make the "key" point of growing season vs. non-growing season, they should have given clear descriptions about how the separation was done accurately and reliably. Lines 29-31: This sentence needs a re-write so that the meaning becomes clear. Line 33: Remove the sentence about latitude and longitude here (the reason is given at line

25). Lines 35-38: Authors need to substantiate about ‘how have they advanced the understanding’ here. Line 53: “on the large scale’? Do you really want to ‘step’ on the large scale by the wall? My guess is that you really want to state: ‘at a large scale’ here. This correction should be made throughout the entire manuscript. Lines 67-68: Move the “and” to the place before the last part of the sentence, before “leaf area index” Line 83: “As known to all, . . .” The sentence is awkward. Line 133 and line 137: How could equations (1) and (2) have the same right sides? Also, what is the time factor for the T here? Is it measured at hourly, daily, weekly or annually time period? Line 155: Please define the “R-square and the model” here. Line 174: Why using “a constant of 0.58” here? I think it should be 0.5 now (see Pribyl 2010, Geoderma 156: 75–83). Line 263: “Q10-ST10” is not shown by Figure 5. Did you mean Q10-ST5? Line 267: Not “Table S3”, should be Table S4. Line 302: “untimely” should be ‘ultimately’ Lines 308-315: The discussion here is unclear. Line 320: “n=20” here, but there were only 6 dots in the figure? Lines 331-352: These low R-square values could be a serious problem for this manuscript. How did you deal with this issue? Lines 405-425: This section is really rough. The quality of the discussion needs improvement. Lines 453-457: To me, Fig.7 actually showed huge differences between those three methods. Lines 471-473: The sentence structure is problematic. Lines 468-481: The Conclusion really needs lots of improvement.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2018-83, 2018.

Printer-friendly version

Discussion paper