

## ***Interactive comment on “Variations and determinants of carbon content in plants: a global synthesis” by Suhui Ma et al.***

**Anonymous Referee #2**

Received and published: 15 September 2017

General Comments: This paper reports the findings of an extensive literature review to determine the carbon content of plants with respect to different organs in individual plants, between plant species and along a latitudinal gradient. While the review is comprehensive, I wonder how these results will be applied in any practical way? The authors present a superficial analysis of how their results are different from canonical values typically used for plant carbon content, but the reader is left to wonder how the results reported here will be used in any practical way?

One concern that I have is that this paper seems ill-fitted to the journal *Biogeosciences*. There's no biogeoscientific data provided and the findings are not discussed in a biogeoscientific context.

Specific Comments: You point out that C content varies across individuals (line 57),  
C1

and that your results suggest that overestimating the carbon content of plant organs could introduce errors ranging between 3.77-13.8% in regional C stock. I wonder if this 3-14% is larger than the variance between individuals, and if not, how much uncertainty does the inter-individual variation add to a regional C stock estimation? Are your findings significant compared to the uncertainty due to different C content between individuals?

Page 7, line 148: Are the differences between your values and those used by the IPCC significant?

While I appreciate the effort to quantify the plant organ C content, if you were to consider the carbon stock of an entire plant, for example a tree, given the % mass that each organ contributes to the overall C mass of the individual tree, is 50% that far off? It's difficult to decipher this from the text, but I would imagine that this is the number that would be of most interest to someone trying to apply this data, for example, calculating a regional carbon pool.

Page 8, line 177: But your results suggest that life form is more important than climate. . .

I'm having a tough time following your argument. If I have this right, life form is the dominant control on C, not climate. But doesn't climate influence life form, particularly along a latitudinal gradient where climate will influence the length of the growing season, water availability, photosynthetically active radiation, etc. . . I guess I don't understand how you can talk about life form independently from climate and attribute it to carbon content. Are you suggesting that within the same species that a latitudinal gradient exists with respect to carbon content? If so, it's unclear.

Technical Corrections: Page 3, line 38: biogeochemical cycling ? Page 3, line 44: ignores Page 3, line 49: compared Page 4, line 66: patterns Page 4, line 71: literatures Page 4, line 77: that used Page 5, line 105: A linear model Page 6, line 106: latitudinal gradient Page 6, line 111: A linear model Page 6, line 125: should it be  $p < 0.15$  and

$p < 0.05$  ? Page 8, line 180: Doesn't this belong in the Results section? Page 9, line 189: shapes the biogeographic patterns. . . Page 9, line 199: "Besides"?

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Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-322>, 2017.