

Interactive comment on “Spatial and seasonal variations of leaf area index (LAI) in subtropical secondary forests related to floristic composition and stand characters” by W. Zhu et al.

W. Zhu et al.

xiangwh2005@163.com

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We thank the anonymous referee for the valuable comments and suggestions. We will carefully take the suggestions into consideration when we revise the manuscript. Our detailed responses to the comments are presented below.

GENERAL COMMENT The topic of the manuscript lies within the field of the journal Biogeosciences. It reports on spatial and temporal variability of the leaf area index in forests. The overall importance of reliable LAI measurements is undoubted and systematic studies of spatial variability within forest stands are seldom. In this sense, the present study is justified. Unfortunately, the description of the methods is insufficient and the obtained results remain therefore questionable. Re: Thanks for the positive

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comments and suggestions on our manuscript. Based on the comments, we will revise the methods and results sections. The detailed responses are presented as follows.

DETAILED COMMENTS Material and methods Since this determines the canopy structure, it should be stated if the studied forest plots were planted or if they are from natural regeneration, further if they were thinned or selectively cut at some point in time. According to the supplementary table S1, it appears that the stands are uneven aged, but a clear information about their history would be useful. Re: The forests are from natural regeneration and no thinning or selectively cutting were applied there till to investigation. We will describe the history of the forests in the revision.

The material used for the hemispherical photography is only poorly described. The camera type is given, but not its manufacturer. There is no information about the lens, not even its viewing angle (or focal length). The choice of picture exposure is not described although it is essential to achieve a good contrast without overexposure. The resolution of the pictures is not given, nor their format. Re: Thanks for valuable suggestion. We will provide clear information about the material of the hemispherical photography, such as the manufacturer (Shiya Scientific and Technical Cooperation, China), the lens (Pentax TS2V114E, Japan), the viewing angle (180°), the picture exposure (automatic exposure set by the manufacturer), the picture resolution and format (768×494 pix, BMP).

The picture analysis is also insufficiently described. There is no indication of the software used, of the pixel classification (thresholding), of the considered viewing angle and if it was divided into rings. The viewing angle would be very important to know here because, in conjunction with the tree height, it determines the integration area of the LAI measurement (which is, in turn, important for understanding the spatial variability). Re: We will revise the manuscript by adding the description of the picture analysis such as the software (the plant canopy analysis software developed by the manufacturer), the pixel classification (thresholding) ($752(H) \times 582(V)$), the considered viewing angle (150°) and it was divided into 5 rings.

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The method to estimate a clumping factor does not state the number of sectors used. The estimation of the contribution of leaves versus wood to the plant area index would be a positive aspect of this study, but here also the methods are poorly described. It is not stated if all of the woody elements on all pictures were painted or only sub-samples. Further, "replace the woody materials with surrounding of non-woody materials" is either a wrong wording or a wrong method. Woody areas should neither be replaced by "non-woody materials" nor by sky pixels, they should be excluded from the analysis because it is essentially not known how much leaf area or sky area they hide. Re: Sorry for our unclear description. We originally described the method according to the reference (Liu ZL, Jin GZ, Chen JM, Qi YJ. 2015. Evaluating optical measurements of leaf area index against litter collection in a mixed broadleaved-Korean pine forest in China. *Trees*, 29: 59-73), where the word "replace" used means "exclude the pixels of woody materials". We will change the sentence into "In Photoshop software, we used the Clone Stamp Tool to select the image of the woody materials (e.g., stems) and excluded the pixels, leaving only leaves on the photos".

Statistical tests are partly done after different types of data transformation. I'm not sure if cutting outliers back to "normally maximal values" is an appropriate method, but at least the measure of this transformation in table 1 should be described in an understandable manner. Using non-parametric statistics would probably make the tests more convincing than the different transformations applied here. Re: Yes, you are right. Our description is not clear, so we will change the sentences into "According to Chiang et al. (2003), we regarded the LAI values as the normal values when the LAI values were within mean value $\pm 3 \times$ standard deviation. Otherwise, the LAI values were outliers and replaced with the maximum or the minimum of normal values. Because the geo-statistics analysis requires that the data meet normal distribution, the transformation was applied if the data did not meet normal distribution". To support our method, we will cite the references (Chiang LH, Pell RJ, Seasholtz MB. 2003. Exploring process data with the use of robust outlier detection algorithms. *Journal of Process Control*, 13(5): 437-449; Dai FQ, Zhou QG, Lv ZQ, Wang XM, Liu GC. 2014. Spatial prediction

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of soil organic matter content integrating artificial neural network and ordinary kriging in Tibetan Plateau. *Ecological Indicators*, 45: 184-194) in the text and add them in the reference list. We hope this revision is clearer than it was before.

Crown coverage is used as a factor in statistical models, but it is not described what this parameter means and how it was measured. A crown coverage is often derived from hemispherical photographs. Is it also the case here, or is it an independent measurement? This can completely change the interpretation of the obtained statistical relationship. Re: Sorry for our ambiguous description. The crown coverage was not derived from hemispherical photographs and it was calculated from crown diameter measured for individual trees within a stand.

The kriging is also insufficiently described in the methods section (it is only in a figure legend that it is given as "ordinary"). The maps produced by this kriging show island structures that probably correspond to the grid of picture taking. If this is true, then it indicates a methodological problem. Either the photographs were systematically taken in some spatial relation to the trees (e.g. on a regular grid in a regularly planted stand). Or the very goal of kriging, i.e. interpolating between discrete measurements, was missed. Re: According to this comments, we will add description of the Kriging in the methods section. Although the ordinary Kriging has the drawback, it is a commonly used interpolating method in the geostatistics reported by other studies (Elbasiouny H, Abowaly M, Abuñij£Alkheir A, Gad A. 2014. Spatial variation of soil carbon and nitrogen pools by using ordinary Kriging method in an area of north Nile Delta, Egypt. *Catena*, 113: 70-78. Dai FQ, Zhou QG, Lv ZQ, Wang XM, Liu GC. 2014. Spatial prediction of soil organic matter content integrating artificial neural network and ordinary kriging in Tibetan Plateau. *Ecological Indicators*, 45: 184-194).

Results and discussion The presented results would probably be interesting, but due to the poor description of the methods they are all more or less doubtful. Re: You are right. We will revise the Methods section (see our responses mentioned above) based on this comment.

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Tables and figures Table 1 and 2 should use the same structure to be comparable. Table 3 should include the sample size, otherwise the column RSS is meaningless. Table 4 gives statistical tests without giving any information on how the different factors affect the dependent variable. Since this is not so easy to put in a table in the case of non-linear relationships, table 4 should make a reference to fig. 3. Figure 1: the two grey tones cannot be distinguished. Re: We will use the same structure for Table 1 and Table 2, and add the sample size in Table 3 as suggested. Based on the comments, in Table 4 we will make the reference to Fig. 3. The two grey bars in Figure 1 will be changed into empty and grey, respectively.

Language The English of the manuscript is well understandable but some sentences are not well structured. At least in one case the wording is inappropriate: "throughout four measurement seasons" would mean at least several measurements in each season (while there is actually one per season). Re: We will revise the manuscript and ask a native English editor to help improve the language.

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