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Interactive comment on "Monitoring of carbon dioxide fluxes in a subalpine grassland ecosystem of the Italian Alps using a multispectral sensor" *by* K. Sakowska et al.

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The paper aimed to estimate gross primary production of subalpine grasslands remotely. Using simple radiometer measuring reflectance in 16 spectral bands synchronously with CO2 fluxes, very valuable data set consists 5 years of observation has been collected. Authors tested two models based on vegetation indices, one of them including incident PAR, and two regression models. The results showed that in vegetation studied, a main factor affecting productivity is total chlorophyll content and, thus, primary production could be accurately estimated via remote detection of chlorophyll content. The results of this study are very interesting and convincing. I believe,

more explicit presentation of the results greatly improve value of this paper.

Firstly, more explanation is required the fact that performance of model 1 (that does use any meteorological data, e.g., incident PAR) is better than model 2 where PAR was used. Recently many studies brought empirical evidence that using incident PAR in gross primary production (GPP) models, requiring meteorological data, does not increase accuracy of GPP estimation. Moreover, the models, which do not use any meteorological information and based only on remotely sensed data, perform better (e.g., Sims et al, 2008; RSE; Yang et al., 2013; GRL). Sakamoto et al., (2011; RSE) showed that the use of vegetation index alone allowed for accurate estimation of crop GPP up to the point where seasonal decrease of PAR became significant. Thus, seasonal change of PAR was found one of the factors affecting GPP. GPP is affected by incident PAR and the response of productivity to change in PAR relates to many factors such as vegetation physiological status and light climate inside the canopy, which affects absorbed PAR and LUE, among others. Therefore, the use of incident PAR in the model may introduce noise and unpredictable uncertainties (see figure below from Peng et al., 2013; RSE, showing it explicitly). As a result, it was suggested using calculated seasonal variation of PAR in the model (Gitelson et al., 2012; RSE). Thus, authors' conclusion that "the photosynthesis process is more efficient under diffuse compared to direct radiation, . . . the accuracy of GEPm estimation decreased after including incident PARm into the model" is only one factor in very complicated interaction GEP/PAR. I suggest to refer Sims et al., (2008; RSE) paper discussing this issue.

Secondly, the performance of the model 1 was very consistent among 4 years of observation (2008-2011); however, it was not a case for 2012. I do not see a problem that "the slopes of these linear relationships in 2011 and 2012 were significantly different from the general model". Slope is not the only factor affecting relationship, there is also intercept. Relationship for 2011 was very close to five year line (Fig. 3). What has to be addressed and explained is very different performance of the model in 2012. I suggest establishing GEP vs. VI relationship for four years (2008-2011) and explain

discrepancy between this close relationship and that for 2012. The reason for this discrepancy is very important to understand; it brings crucial information about validity of the model.

Thirdly, in discussion authors should address limitations of the applied models. I am not sure that authors used right expression ("simultaneous estimates of ε can be redundant") about necessity to assess light use efficiency in non-stressed ecosystems characterized by strong seasonal dynamics such as grasslands and croplands. But why "non-stressed" vegetation mentioned? Authors study natural vegetation that does stressed. LUE relates to electron transport that in turn relates to chlorophyll content. Thus, detecting chlorophyll content does help to take into account some aspects of plant physiological status but there are many other factors affecting plant status and, thus, assessing LUE is extremely important especially for natural stressed vegetation. Obvious lag between stress and decrease in chlorophyll content does affect accuracy of the model and it should be explicitly mentioned.

Forth, I suggest authors to select only figures those are really necessarily for clear and understandable presentation obtained results. These results are valuable and would be much better presented by selecting few self-explained figures.

Finally, abstract does not seems to me very informative and conclusions requite thoughtful revision.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/11/C933/2014/bgd-11-C933-2014-supplement.pdf

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