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Interactive comment on "Investigating the usefulness of satellite derived fluorescence data in inferring gross primary productivity within the carbon cycle data assimilation system" *by* E. N. Koffi et al.

Anonymous Referee #1

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The opportunity of using solar induced fluorescence (SIF) data obtained from satellites opens new perspectives in the study of vegetation-atmosphere interaction. In fact, differently from reflectance, fluorescence is produced by plants as direct result of their biological activity, so it has the potential of being a direct measure of photosynthesis. The efforts aimed at implementing SIF data into biogeochemical modeling have therefore a great potential in global carbon cycle studies. The CCDAS model used in the present work by Koffi and colleagues, with his prognostic capabilities, is apparently an adequate tool for testing SIF data potential.

In the current study, the Authors tried to implement into CCDAS a module coming from another model, SCOPE, and to derive fluorescence and gross primary productivity at regional and global scale, essentially moving from absorbed photosynthetically active radiation (aPAR) data and chlorophyll content information.

Results are realistic in terms of modelled GPP and SIF, but is worth mentioning that both variables are linked with aPAR, so it is not clear what is the real improvement coming from of the current modeling effort. It seems to me instead, that equifinality exists between fluorescence and aPAR (and possibly also with chlorophyll content and Vcmax). I'm possibly biased toward data-oriented semi-empirical models, but I would find more interesting to see model outputs obtained using SIF measured data as an input, possibility mentioned at the end of the paper.

There are also aspects not fully convincing in the model output, like the relation between chlorophyll content and GPP (Fig. 2d), apparently contradicting the well established positive relationship between the two variables (Fleischer, 1935). This is acknowledged at the end of the paper, where a potential effect of Nitrogen content is also invoked, but highlights anyway the limits of the current modeling exercise.

What I find to be missing, in order to properly evaluate the results from the current modeling activity, is an independent testing of the results in terms of modelled GPP. Since thousand of site-years of GPP data are available from the FLUXNET database, I strongly recommend to perform model-data comparison in a revised version of the paper, instead of using a single period of a FLUXNET site (probably Cabauw, NL, 2006) in which GPP data were not available, as in the current version of the paper.

Specific comments

The abstract is extremely short and not very informative: which are the main findings from the current study?

Page (P) 708, Line (L) 16-18. The term 'system' is repeated three times, probably with

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different meanings. As a result, the sentences are somewhat hermetic.

P708 L21: 'Recent work have': Please check grammar.

P709 L24, P710 L2: 'data are', 'data is': Please be consistent.

P710 L1: The CCDAS model is clearly presented in Scholze et al., 2007, while here information on its structure is missing. I prefer self-standing papers, so I recommend a short overview of that model also here.

P711L27: 'The vegetation is characterized by different values of the leaf area index (LAI).' I guess the Authors refer to the parameter vector representing vegetation.

P713L1: 'from absorbed fluxes': which kind of fluxes?

P716L7: '13 plant functional types (PFT) based on Wilson and Henderson-Sellers (1985)': Unfortunately, these 13 plant functional types are not defined elsewhere in the text, and in the mentioned paper from Wilson and Henderson-Sellers a total of 80 land cover classes and 8 grouped land cover classes were defined.

P718: 'vapour pressure'. It is not clear to me which kind of information the Authors try to capture from vapour pressure information. In plant physiology it is well established a linkage between the vapour pressure deficit (dew point water vapour pressure-observed vapour pressure) and stomatal conductance and also GPP (e.g. Duursma et al., 2014), but I'm not aware of a direct link between plant physiological responses and air water pressure. The same at P723L7 and in Fig. 4a.

P722L7: Since aPAR appears to be a key variable in this modelling, it is not clear to me why the authors do not show it in the graphs.

P725 L20-21: 'regional. . .regions'? Please correct.

P729L19-20: 'Any model seeking to use Fs should therefore account for chlorophyll concentration.' I think that the Authors are doing a merely inductive reasoning while making this statement. They tested a single model only, indeed.

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P 731L15-16: ‘We have seen a strong linear relationship between the fluorescence Fs and aPAR.’ Is this an observational result or a modeling result?

Figure 4. Looking at the main x axis and at the represented daily patterns, it seems that both GPP and fluorescence have a peak at midnight in the second day of the time series. It looks strange.

Captions of Fig. 5 are really unclear, consider rewriting.

Figure 6 is difficult to be understood, since the colours representing the different PFTs are not defined.

Cited References Duursma R.A., C. V.M. Barton, Y.-S. Lin, B. E. Medlyn, D. Eamus D.T. Tissue, D. S. Ellsworth, R. E. McMurtrie, (2014) The peaked response of transpiration rate to vapour pressure deficit in field conditions can be explained by the temperature optimum of photosynthesis, Agricultural and Forest Meteorology 189–190, 2–10.

Fleischer, W. E. (1935). The relation between chlorophyll content and rate of photosynthesis. J. Gen. Physiol., 18: 573-597.

Scholze, M., Kaminski, T., Rayner, P., Knorr, W., and Giering, R. (2007). Propagating uncertainty through prognostic carbon cycle data assimilation system simulations, J. Geophys. Res.,112, D17305, doi:10.1029/2007JD008642.

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