We warmly thank the editor and the 2 reviewers for their constructive comments on the revised version of the paper. In what follows, the comments of the editor/reviewers on the revised version of the paper are in italic and our reply in normal face. The pages and lines indicated in our responses are those in the revised version of the manuscript, otherwise it is specified. Also, the changes in the text are highlighted in red.

Associate Editor Decision: Publish subject to minor revisions (Editor review) (10 Jun 2015) by Georg Wohlfahrt

Comments to the Author:

The authors have reasonably responded to the reviewer comments, in particular the inclusion of the comparison against FLUXNET GPP improved the paper. This is acknowledged by both reviewers to which I have sent the revised paper for a second review given they had indicated earlier that they would like to see the revised paper.

After having giving the paper a read myself I largely agree with the reviewers in that another round of minor revisions will be necessary before the paper can become acceptable for BG. In addition to the reviewer comments I detected quite some additional issues that need to be taken care of before the paper can become acceptable for publication.

I thus ask the authors thus to consider the reviewer and my comments below and upload a correspondingly revised version together with a point-by-point reply to ALL comments and the corresponding changes.
The responses to the all the comments are given hereafter

Overall the English of the paper is quite poor at times – this however will be taken care of during copy-editing once the paper has been accepted. Many of the figures in my view are in the need of careful editing to make them more appealing, e.g. distance between axis text and axis numbering, number of ticks per axis, overlapping ticks in case of multiple axis, legend text overlapping with figure elements, consistent use of units and headers, size of panel numbering, … –it is the authors responsibility to produce figures which meet the journal's standards and I ask the authors to consider this when submitting a revised version.
The figures and legends have been revised as asked

Details
p. 2, l. 3: "with the advent of"
Corrected. See page 2, line 3

p. 2, l. 6: remove the last part of the sentence „…, the downward flux …"
Corrected as suggested. See page 2, line 6

p. 3, l. 2: remove "natural"
Removed as suggested. See page 3, line 2

p. 4, l. 14: "sun-induced plant fluorescence"
Done as suggested. See page 4, line 14

p. 6, l. 21: before explaining what you do, please explicitly state the objectives and hypothesis (if any) of the study
The text above this line has been revised as follows:
"To assess the usefulness of satellite based fluorescence data (SIF) to constrain GPP within CCDAS, in this study, we investigate the sensitivities of both GPP and SIF to the biochemical parameters as well as environmental conditions by using the SCOPE model alone and the forward mode of the CCDAS built around it." See page 6, from line 17

p. 8, l. 18: this is wrong- you may use any leaf angle distribution describing it by the parameters LIDFa and LIDFb – presumably you have chosen these parameters to result in a spherical distribution!
Right. Effectively, in this study we use LIDFa and LIDFb values for a spherical distribution. These values are reported in Table 2, which is now quoted in this sentence. See page 8, from line 18

p. 9, l. 23: what does Kn represent – explain
Kn is a rate coefficient relative to nonphotochemical quenching (NPQ), a parameter obtained from Pulse amplitude modulated (PAM) fluorometry. PAM measures the photosynthetic efficiency of photosystem II (PSII). These sentences are put in the text at page 9, from line 22

p. 11, l. 2: summation is somewhat misleading as the TOC fluorescence depends on the within-canopy radiative transport of radiation in the fluorescence wavebands
This has been revised as follows:
"The total top-of-canopy fluorescent radiance is obtained from the fluorescence flux (i.e., Φ_{Ft} in Equation 1) and the spectral radiance of single leaves over all layers and orientations, taking into account the probabilities of viewing sunlit and shaded components… ". See page 11, from line 5

p. 12, l. 19: isn't the LAI implicit in i) in l. 16 ?
Yes. The item iii) has been then deleted and the text has been revised accordingly. See page 12, from line 21

p. 13, l. 6: "residual differences" – this sounds like a misnomer – you mean small/negligible differences?
"residual" is replaced by "negligible" as suggested. See page 13, line 10

p. 14, l. 21-22: sparse and dense are not necessarily good attributes for the different LAIs you simulate as sparse usually means heterogeneous horizontal structure and dense actually refers to the volume (with a LAI=4 a canopy 1m tall canopy is denser compared to a 10m tall canopy) – I suggest to remove this sentence as it is quite obvious why you chose different LAIs
The sentence has been deleted as suggested. See page 14, from line 24

p. 14, l.22-24: what do you mean with temperature and vapour pressure at leaf level? Is it the (surface) temperature of the leaf and the (saturated) vapour pressure in the intercellular space of the leaf or the air surrounding the leaf? If the vapour pressure refers to the leaf intercellular space, then there is no need to specify this as this is a function of leaf temperature (assuming saturated conditions) – please clarify this sentence in a manner accessible to ecophysiologists – one of the reviewers was also commenting on this.
It is about the atmospheric pressure, temperature, and vapour pressure surrounding the leaf. This has been clarified. See page 14, from line 25

p. 14, l. 25-p. 15, l. 1: this repeats what you write above
The sentence has been deleted. See page 15, line 3

p. 15, l. 9: "to the chlorophyll AB content"
Corrected. See page 15, line 11. Also, corrected throughout the text and the captions of Figures/Tables when relevant

p. 16, l. 21-22: where do you then get leaf temperatures from – Bethy?
The temperatures of the air surrounding the leaf in the CCDAS are from BETHY. These temperatures are used to approximate the leaf temperatures in SCOPE. For each pixel of SCOPE (hence BETHY), we have this data at monthly scale as described in the Section 2.2.2 of the paper. This has been clarified. See page 16, from line 25

p. 17, l. 16: replace "when the electron rate is active" with "when photosynthesis is limited by electron transport" as electron transport is always active when there is light
Corrected as suggested. See page 17, line 21

Fig. 2, panels e and f: what does the x-axis show – radiation in the visible range (400-700nm)? Based on the values I rather have the feeling the plot is showing short-wave radiation, i.e. ca. 400-2500nm – please confirm and if so change figure and text accordingly
Right. Effectively, the x-axis is showing the broadband incoming short wave radiation (400-2500 nm). The figures/captions and text have been revised accordingly. As an example for the text, see at page 15, from line 15

p. 26, l. 19: Fig 8c instead of Fig. 9c
Corrected. See page 27, line 18

p. 27, l. 24: "Discussion"
Corrected. See page 28, line 23

p. 30, l. 11; aPAR of 1400 W/m2 is likely impossible – either this is not PAR (rather global radiation) or the units are wrong (e.g. umol/(m2s) instead of W/m2); the conversion of PAR between photon and energy units is 4.5 – so for an incoming PAR of 2000 umol/(m2s) / 4.5 = 444 W/m2 in terms of incident PAR in energy units – aPAR will be correspondingly lower

Right. This was a typing mistake. Effectively, APAR is expressed in μmol/(m2s). This has been corrected. Following the suggestion of the reviewer #2, this text has moved to page 22, line 5

p. 30, l. 20: the original version of SCOPE does calculate aPAR based on LAI and incident PAR and so forth– it is not an input to SCOPE
Yes, but here it is about the CCDAS. Effectively, aPAR is calculated by the radiative transfer module in SCOPE, but it is an input variable of the biochemical routine in SCOPE. For the CCDAS, aPAR is considered as an input since it is used in the biochemical model with the other parameters to compute e.g., GPP and the fluorescence flux. This has been clarified as follows:
"aPAR is an external forcing for the biochemical modules of the biosphere model (e.g., SCOPE or BETHY) ….". See page 31, line 4

Supplementary material: Numbering of figures and tables should start with 1 and include the "S", i.e. Fig. S11 should be Fig. S1
This has been revised as asked. Also, almost all the Figures have been revised

Referee #2

Report on the revised version of ''Investigating the Usefulness of Satellite derived Fluorescence Data in Inferring Gross Primary Productivity within the Carbon Cycle Data Assimilation System', by Koffi et al.

The Authors did a considerable work to improve the previous version of the manuscript. However, it seems to me that there are still some errors inside and I recommend an additional iteration of the revision if possible. I add that overall the paper is still quite weak in terms of clarity.

Remarks:

Page 11: The Authors insist in referring their 13 PFTs to a wrong paper (Wilson and Henderson-Sellers (1985))

The 13 PFTs were derived from the PFTs reported in Wilson and Henderson-Sellers (1985). This was performed during the PhD thesis of Wolgang Knorr. The sentence has been rephrased as follows:

"CCDAS uses 13 plant functional types (PFT; see Table 1), which have been derived by a condensation (grouping different crop types into one crop PFT) of the original 23 PFTs in BETHY (Knorr, 1997, based on Wilson and Henderson-Sellers, 1985). See page 11, from line 25

Reference: Knorr, 1997, Satellite Remote Sensing and Modelling of the Global CO2 Exchange of Land Vegetation: A synthesis Study, PhD thesis, Max-Planck-Institute for Meteorology, Hamburg, Germany

Page 15: It seems that the Authors choices for the data to be used for comparison are unfortunate: in this case, measured SIF data are missing. Why not to choose another time period?

For the selected stations, unfortunately there are no observed SIF data from the FLUXNET database we have at our disposal. However, for this work, we will not consider any additional analyses. We think that a throughout validation of both GPP and SIF of SCOPE is still needed, but this is beyond the scope of this work. Consequently, the text remains unchanged

Page 17. 'A moderate positive relation is found between SIF and GPP....', I cannot find this in the graphs.

We have a correlation between SIF and GPP for V_{cmax} values less than 125 μmol m^{-2} s^{-1} (not shown). Effectively, from the graphs, it is not obvious to see that. However, we can see that both SIF and GPP increase with V_{cmax} values less than 125 μmol m^{-2} s^{-1}. The text has been clarified. See page 17, from line 24

Page 20: 'The small variations in GPP at certain episodes can be explained by the temporal variations of both the temperature (Figure 4a)'. I cannot understand.

It is about the temporal variations of the temperature. The word "both" has been deleted. See page 20, from line 16

Page 24: 'Flexas dataset', Are the Authors referring to Jaume Flexas? It doesn't seem to me the correct way to refer to a person.
The authors are now correctly referred. The text has been clarified. See page 25, line 10

Reference: Flexas, J., J. M. Escalona, S. Evain, J. Gul´ıas, I. Moya, C. B. Osmond, and H. Medrano (2002), Steady-state chlorophyll fluorescence (Fs) measurements as a tool to follow variations of net CO_2 assimilation and stomatal conductance during water-stress in C3 plants, Physiol. Plant., 114(2), 231–240.

Page 25: 'Some of this mismatch corresponds to unlikely locations for satellite-derived SIF, e.g. central Australia.' It seems to me that is the modelling to be unlikely, not the satellite SIF. Central Australia is almost a desert.
This has been revised as suggested, i.e., the modelling and not satellite. See page 26, from line 15

Page 30: Results of the comparison with Zhang findings would be more conveniently presented in the Results section.
The comparison of our results with those from Zhang et al. (2014) is now put in the results Section (here Section 4.1. idealized sensitivity using SCOPE). Also, the text has been revised. See page 21, from line 20

Page 33: 'uncertainties in the radiation'. Which kind of uncertainties?
For the CCDAS, in this study, we used the short wave radiation from WATCH database which is based on the ECMWF (ERA-40) reanalyses (Weedon et al., 2011). Then, one can estimate the uncertainties on this data when comparing them with observations from e.g. FLUXNET.

Page 44: 'several leaf area index', possibly, leaf area indexes (or indices).
Corrected as suggested by "indices" throughout the text and the figure captions. See e.g., at page 45, line 23

Page 45: 'The observed GPP from is in black'. From what?
"GPP is in black". The word "from" has been deleted. See page 46, line 16

Caption of Figure 5: 'are show'->are shown.
Corrected. See page 46, line 22

Caption of Figure 6: 'Correlations between CCDAS simulated quantities and between simulated quantities...'. I believe that more care is needed in the writing, indeed.
Correlations between CCDAS simulated quantities (i.e., SIF, GPP, and aPAR) and between these simulated quantities and satellite GOSAT based fluorescence SIF are shown. This has been clarified in the caption of this figure. See page 47, from line 11

I recommend also to consider the following recent paper:
Yang, X., J. W. Tang, J. F. Mustard, J. E. Lee, M. Rossini, J. Joiner, J. W. Munger, A. Kornfeld, and A. D. Richardson. 2015. Solar-induced chlorophyll fluorescence that correlates with canopy

photosynthesis on diurnal and seasonal scales in a temperate deciduous forest. Geophysical Research Letters 42:2977-2987.

We quote this paper in the discussion when we recommend the use of an empirical method between GPP and SIF (Section 5). We then add this sentence: “Moreover, Yang et al. (2015) when investigating a temperate deciduous forest, they found that SIF incorporated information about both aPAR and light use efficiency (LUE), the two main components of GPP”. See page 31, from line 16

Referee #4

This is a review of the re-submitted manuscript. The revised manuscript improved in terms of clarity and structure. I appreciated the authors' hard work in addressing these comments. However, several details still require attention before publication.

The authors have made some additional simulations and analysis. First, I agree with the authors, at the current stage of CCDAS, an empirical relationship between SIF and GPP would be more practical due to the uncertainties of model and dataset (e.g., LAI, Chl) especially at regional and global scale. However, the conclusion of the sensitivity of SIF to Vcmax need to be further discussed. As shown in their Fig. S12, it would not be a 'slightly increase' of SIF to Vcmax to low light to high light conditions.

The text in the Supplementary material (Section 4) "slightly increase" has been changed in " increase"

Meanwhile, due to the ongoing development of fluorescence model, the different versions of SCOPE are actually slight different in some parts. Based on our experiences, the new version of SCOPE actually shows less sensitivity of SIF to Vcmax than previous versions. However, higher sensitivity still exists under high light conditions. Actually the authors' results support this statement. From their additional sensitivity analysis in Supplement, they found a factor of 2 in new version SCOPE while Zhang et al. (2014) found a factor of ~3.5 with old version. Ignoring the differences due to versions, can we say a factor of ~3.5 is a strong sensitivity, but a factor of 2 means a weak sensitivity under high light conditions as they stated in the Conclusion part ?

On the other hand, the weak sensitive of SIF to Vcmax under low light conditions is actually consistent with that of Zhang et al. (2014). In their Figure 3, the sensitivity of SIF to Vcmax is weak during the early or late growing season which is low light conditions.

We agree that the variations of SIF with V_{cmax} from low light to high conditions are well reproduced by the two studies. However, the amplitudes of the sensitivity of SIF to V_{cmax} in the two works are different. We still think (based on our results on the paper and the Supplementary material) that SIF is weakly sensitive to V_{cmax} and this sensitivity increases under high radiation conditions and for lower V_{cmax} ranges. Consequently, we do not amend our conclusions. Indeed, for us having SIF difference of 2 $\mu mol.m^{-2}s^{-1}$ over 10-200 $\mu mol.m^{-2}s^{-1}$ V_{cmax} range is (unfortunately) a weak sensitivity.

The comparison of our results with those Zhang et al. (2014) is now put in the results Section (here Section 4.1. idealized sensitivity using SCOPE) as suggested the reviewer #2. See page 21, from line 20

Soybean is not C4 crop but C3 crop (Section S4 and Fig. S41)! Please make sure the right model is used for soybean.

Right. The simulations for soybean are now performed by using a C3 plant in SCOPE instead of C4, as previously shown. The Fig. S12 has been updated accordingly (See Section 4 in the Supplementary material).

P2, Line 15: Please consider rephrase as 'Low sensitivity is found of SIF to Vcmax under low light conditions, …'.

The sentence has been rephrased. See page 2, from line 14

P2, Line 22-23: Please consider as '…, the limitations of … in the present set-up in CCDAS system.'

The sentence has been rephrased. See page 2, from line 21

P5, Line 11: Should be 'Lee et al., (2013)'. One of the reviewers has already pointed out this last time. Please correct it.

Corrected. See page 5, line 11

P30, Line 4-5: As I pointed out already last time, how the 4 times differences come from? The author's response did not really clarify this aspect as well. Considering the different version of model used, different configurations and wavelength used, it's not clear how to get this number. Meanwhile, their sensitivity analysis in Supplement doesn't support this. Please consider remove or rephrase this sentence.

In fact, we first compared our results (SIF at the frequency 755 nm) to those from Zhang et al. (2014). Then, we compare our simulations by using the settings of Zhang et al. (2014) with those from Zhang et al. (2014). For the comparison of our results (i.e., SIF at the frequency 755 nm) with those from Zhang et al. (2014), we found that the results of Zhang et al. (2014) [here their Fig.3 for soybean and for August 13) are about 4 times our sensitivity of SIF to V_{cmax} in the range of 10-200 $\mu molm^{-2}s^{-1}$ as shown in Figure 3a (this paper)." The text has been clarified. See page 21, from line 20

P32 Line 17-18: This conclusion should be consistent with that in the Abstract.

This sentence has been rephrased as follows :

"As expected, GPP is strongly sensitive to V_{cmax}, while SIF is more sensitive to C_{ab} and only weakly sensitive to V_{cmax} under high radiation conditions and lower V_{cmax} ranges." See page 33, line 4