

Interactive comment on “Tree-grass phenology information improves light use efficiency modelling of gross primary productivity for an Australian tropical savanna” by Caitlin E. Moore et al.

Caitlin E. Moore et al.

caitlin@moorescience.com.au

Received and published: 24 August 2016

Authors' response to reviewer comments for manuscript bg-2016-187 “Moore et al., Tree-grass phenology information improves light use efficiency modelling of gross primary productivity for an Australian tropical savanna”

We wish to thank all three reviewers for their helpful and constructive comments regarding our manuscript. Their comments are relevant and we feel will improve our manuscript. Below we outline our response and the way in which we have addressed each of their comments.

[Printer-friendly version](#)

[Discussion paper](#)



Firstly, I would like to congratulate the authors to a very interesting and well written manuscript. I truly enjoyed reading it and I learned a lot. However I have some questions that I would like to get answered before I can recommend this manuscript for publication:

This manuscript is not focusing on the EC based understory estimates of CO₂ fluxes, but I am still confused. The eddy covariance method is based on the assumption that measurements are done in the inertial surface layer, i.e. in the layer within the atmosphere where there are no vertical changes in fluxes depending on height of the sensors (Foken, 2008). This is not the case inside a canopy. Inside a canopy turbulence is very chaotic, and turbulent transport is much more efficient than above a canopy (Denmead and Bradley, 1987; Foken, 2008; Kaimal and Finnigan, 1994; Raupach, 1989). Additionally, there are sinks and sources in all directions in space. Fluxes can thereby basically come from any direction; the NEE estimated by the EC system is thereby not only a result of fluxes from the understory, it can equally as well be a result of respiration or CO₂ uptake by the canopy cover above the EC system.

The understory EC data used in this study is that which is already published in the same special issue of Biogeosciences (Moore et al., 2016). This paper discusses and validates the use of an understory flux tower for savanna research in more detail. In particular, it presents results from a cospectral analysis, based on the work of Kaimal and Finnigan (1994) (as referenced by the reviewer), to show that the flux tower does primarily record vertical transport during daytime turbulent conditions. We referred to Moore et al. (2016) within section 2.2 to direct readers to this more detailed discussion. Given all three reviewers commented on the length of the manuscript, we feel it would be ineffective to elaborate on this further. However, we will ensure that more explicit reference is made to Moore et al. (2016) within section 2.2 in order to help resolve some of this confusion.

Interactive
comment

Printer-friendly version

Discussion paper



(P8 L20) In case you do not include the reflected PAR in the estimates of fraction of absorbed PAR (FAPAR), it is not FAPAR, it is the fraction of intercepted PAR (FIPAR). This is not the same thing. Generally, FIPAR is much more stable over the seasons than FAPAR, and this can make a difference in the estimate of the seasonal variation in GPP. Why did the reflected PAR data result in negative values during the dry season? It indicates some issues with the calibration of the sensors. Is there no way to inter-calibrate the sensors and recalculate the data? FAPAR is generally estimated as:

$$\text{FAPAR} = \text{PAR}_{\text{in}} - \text{PAR}_{\text{ref}} - (1 - \alpha) \times \text{PAR}_{\text{tr}} \text{PAR}_{\text{in}}$$

Where PAR_{in} is incoming photosynthetic active radiation (PAR), PAR_{ref} is reflected PAR, α is PAR albedo of the soil, and PAR_{tr} is PAR transmitted through the vegetation.

The reviewer is correct here and given reviewer 2 and 3 also highlighted this point, it prompted us to re-check our analysis of fPAR and APAR.

Initially the reason for omitting reflected PAR was due to fPAR values often being negative in the understory in the late dry season. This was most likely due to the lack of vegetation in the understory in the late dry season around some of the towers, which caused incoming PAR below the understory to be almost equal to that of PAR above the understory. For one tower, PAR below the understory was higher than PAR above the understory, which is a result of the heterogeneous nature of the savanna ecosystem at these point scales. By omitting this tower from the analysis during the late dry season, negative fPAR values no longer occurred in the understory. This data was then used to calculate APAR, not IPAR.

Basically, there was a bug in the code that was missed on previous checks before submission. This bug was due to an incorrect labelling of the APAR variable to an alternative version, which omitted the reflected/upwelling PAR to test the above theory about the negative fPAR values. Therefore, we incorrectly concluded that by omitting the reflected PAR, the model performed better, when in fact it was actually using the correct, reflected PAR-included APAR values.

[Printer-friendly version](#)[Discussion paper](#)

To sum up, we are grateful for the keen eyes of all three reviewers here for picking up on this mistake before the manuscript made it further in the review process. Thankfully, the data presented are correct, they were just interpreted incorrectly on our behalf, so we will amend this in the resubmission.

I do not understand how the model can overestimate the GPP? You estimate a maximum LUE based on an average LUE for Dec-Mar. Then you use scalars with a value of between 0 and 1 to downscale the maximum LUE to a lower value. But since maximum LUE is based on the same time series of GPP as you use for the evaluation, it should not be possible for modelled GPP to be overestimated. Or did I misunderstand something? Please clarify.

We are a little unsure as to what the reviewer is referring to with this statement, if it is one aspect of the text/figure or if it is our general approach to our research question. However, to answer this query at a general level, the model can most definitely overestimate GPP (or underestimate it) as LUE is not the only input to the GPP model. APAR is also an input, which in the case of the savannas is often overestimated during the transition periods between wet and dry seasons (i.e., Kanniah et al., 2009, Whitley et al., 2011). Meteorology also drives the down-regulation of maximum LUE to daily LUE variability, so although we obtained maximum (peak) LUE from our GPP estimates, the application of this down-regulation process means the two parameters are no longer directly related. Therefore, by using APAR and LUE in the model, GPP can be over- or underestimated. This is why we chose to test whether including phenology information would improve the model's ability to capture flux tower GPP, given this savanna ecosystem displays such a distinct boom-bust seasonal phenology.

Specific comments: L11, it sounds like all grass in savannas is C4 species, which is absolutely not the case. Please just rephrase a bit.

[Printer-friendly version](#)[Discussion paper](#)

We will fix this in the line identified _____

P6 L1 Please describe very shortly the partitioning method used. Was it based on a light response curve or night time NEE-temperature curves?

We used a u^* filter and artificial neural network approach, with soil water, soil temperature, air temperature and EVI as the main model drivers, to determine respiration (R), assuming all night time NEE was R. This was extrapolated to the daytime and GPP was calculated as the difference between R and NEE. Further information about this process can be found in Beringer et al. (2016), also an article in the special issue our manuscript is a part of. Hence, we will add this short description to P6, L1 and direct the reader to Beringer et al. (2016) for further information.

Generally in the method section there are very many technical details. These are nice to have, but I think they could be moved to supplementary material to ease the reading of the manuscript. But, it is ok the way it is now as well, it is just a suggestion.

This is a good suggestion, and given the other reviewers have also suggested this, we will revise the manuscript and shorten where possible.

P9L18 APAR is in MJ d-1.

We will fix this in the text. _____

P5 please indicate the study period of the EC measurements, and other measurements by the way.

The study period was from 12th December 2012 to 14th October 2014, for all measurements. We will add this to the text. _____

P9 L24 Why is $n=8$? In the figures it looks like the measurements started in January 2013, which would mean $n=7$?

Printer-friendly version

Discussion paper



N=8 because it includes the months of Dec through to Mar (inclusive), which each occur twice during the study period. We stated this in the text on P9, L22, but could make it clearer in the revised version. _____

P9 L22 Why did you bin the LUE to months, this does not necessarily give the best indicator of maximum LUE. I would say that better would be to use a running mean for the estimates of seasonal dynamics in LUE, and then use the maximum value. Why should the average of 3 months give the best estimate for a maximum?

We binned LUE by month and termed it peak LUE, rather than maximum LUE, because true maximum LUE is not easy to obtain from EC measurements. What we wanted to get at was a representative maximum LUE that was obtained during conditions that were not limiting to growth. A similar approach was used by Kanniah et al. (2009), so we intended to mirror their approach in terms of calculating a maximal LUE estimate from EC measurements. Perhaps better would be to describe LUE as “optimum” rather than “peak”, as peak suggests it is an instantaneous maximum rather than an overall optimum value.

P16 L 17 Why did you use GCC as a proxy for FAPAR, and not as a scalar for LUE? There is strong seasonal variability in LUE depending on phenology of the vegetation, so I would think that it is more realistic to use the phenology as a direct scalar on LUE.

We used GCC as a proxy for fPAR because the high values of fPAR in the transition periods were what we believed to be the source of the error in the model. LUE reduces rapidly from Feb to May, which is more characteristic of the phenology response seen in the field (i.e. Figure 3). Given this, the LUE was more indicative of phenology driven GPP than APAR, so was less likely to be the source of the error in the model than APAR. _____

P10 L29 I assume that the regression was not used to replace APAR, but to replace

[Printer-friendly version](#)[Discussion paper](#)

FAPAR?

The phrasing of this sentence is misleading. It should read “Daily EVI were regressed against site-based daily ecosystem fPAR, and the regression was used along with incoming PAR information to replace APAR in Eq. (6).”

P12 L34 What limitations?

The limitations refer to those mentioned in line 29 of the same paragraph. Perhaps better would be to refer the above mentioned uncertainty. The suggested additional analysis from reviewer 2, once added here, should also help resolve this confusion.

P13 L4 I would not consider a R2 value of 0.09 and 0.23 a well correlated relationship. These relationships are not well correlated just because the p-value is significant. The assumptions for testing of significance is not fulfilled; there is high auto-correlation present in eddy covariance time series, so the true N is nowhere near the observed N. For example, Desai (2014) addresses this issue using a reduced degree of freedom calculation to show that the vast majority of flux tower regression is actually overconfident.

We agree with the reviewer here, better would be to say more broadly that the relationship was stronger for the understory than for the overstory. We will change this in the text accordingly. _____

Fig 8-10. I suggest to incorporate subplots just like you did in Fig 7. Where you include a subplot with modelled GPP on the y-axis and the measured GPP on the x-axis. This really helps to see how well the models perform.

This is a good idea. The reason we did not do it from the beginning was because we felt it made the figures too busy, so we included this information in Table 2 instead. However, we do agree that it would add to the figures, so we can include it in our

BGD

Interactive
comment

Printer-friendly version

Discussion paper



resubmission. _____

P15 L25-L27 Are you certain that RMSE is higher for the GCC included model (RMSE =1.43) than for the GCC and EF combined model(RMSE=1.36)? When looking at Fig 8 it does not look like RMSE can be higher. In Figure 8, it looks like the errors are much smaller; this should also be seen in the RMSE values.

We can see what the reviewer means in regards to the RMSE values in Table 2 vs. the timeseries in Figure 8. We have double checked our values and have found that some values in Table 2 need amending, which we will do in the resubmission. However, for the overstory, the RMSE value is lower for the LUE_EF_GCC model when compared with the LUE_GCC model, despite what Figure 8 indicates. What we think has caused this is that while the error appears enhanced in the wet season in the LUE_EF_GCC combined model, it is reduced in the dry season, which results in RMSE being slightly lower than for the GCC model alone. By adding the scatterplots to each of these figures, as suggested by reviewer 1, this may help to reduce some of this confusion.

Reviewer #2

The use of GCC in the LUE model is thought to improve the GPP estimation because of the strong phenological cycle of the target. In my opinion the phenological cycle is very well represented when fAPAR is used. So the reason for using GCC must be different: replacing fAPAR measurements or testing if a “green” index (likely a proxy of a “green” fAPAR) provides a better description of photosynthesis than that of total fAPAR.

The reviewer raises a valid point here, in that fAPAR does capture the phenological cycle reasonably well. However, it does not capture it perfectly and is particularly poor during the transition from the wet to dry season (or dry to wet). We believe this is due to the senescence of the understory grasses that changes the greenness and

BGD

[Interactive
comment](#)

[Printer-friendly version](#)

[Discussion paper](#)



GPP of the savanna despite fPAR remaining high. Currently, savanna productivity models poorly capture this change (i.e., Kanniah et al., 2009, Whitley et al., 2011, Whitley et al., 2016), and we would argue it is because they do not capture the understory phenology dynamics as well as they could. Moore et al. (2016) found that the understory accounts for 1/3 of savanna GPP, which is heavily dominated by the annual grasses that show this strong phenology. When models only use the fPAR (or APAR) information, they fail to capture the transition from wet to dry (and dry to wet) and over-estimate GPP. By using the GCC information, which provides a more accurate representation of phenology when compared with APAR for this savanna, the LUE model performs better. Ma et al. (2014) also reached a similar conclusion when they used EVI to incorporate better phenology information into their GPP model.

Cameras pointing to trees: as large part of the ROI is occupied by the background (the sky), I wonder if the observed (and reduced) variability in GCC is not related to variations in sky optical properties during the year. The relation with LAI (Fig 6b) is not helping to figure it out, as the observed relation between GCC and LAI may be spurious (i.e., LAI increase and decrease in parallel to changes in sky optical properties). To disentangle the two effects it would be useful to define some additional ROIs with sky only and analyse the difference with the tree-ROIs selected.

This is a really good idea and we will endeavour to analyse a sky-only ROI to compare against our overstory estimates. We will include a discussion of these results in the revised manuscript and add the sky timeseries to support figure 6.

Performances of the different GPP models (4, all LUE based) are assessed in terms of r , RMSE and RPE. However, model 1 and 2 (eq 6 and use of EF) are used in prediction while (if I got it well) model 3 (using phenocam index) is in fitting (as two parameters, m and c coefficients) are adjusted. Model 4 (using MODIS) is in between, because a relationship is tuned between EVI and fAPAR. Therefore, results are not comparable in

[Printer-friendly version](#)[Discussion paper](#)

my opinion (see the discussion at page 15).

Each model combination is compared against flux tower-derived GPP estimates and the r , RMSE and RPE provide an indication of which model is best at capturing tower GPP. Our discussion on pg 15 discusses which of the model combinations was best at capturing tower GPP, finding that the inclusion of phenology information did the best job.

An interesting point is that the use of the phenocam index appears to eliminate the lag between measured and modelled GPP. The reason for this could be that the total fAPAR used by the other model is the source of this lag. On the contrary GCC may represent a kind of “green” fAPAR that is more in line with photosynthesis. A dedicated section comparing phenocam indexes and fAPAR would be very useful.

We can address these concepts in the discussion, which we feel will also help to address the queries raised by this reviewer about the use of GCC as a better indicator of phenology in the model, as identified above.

Specific comments:

1 L 32 r2 ranging from 0.1 to 0.2 (overstory) is much lower than that of understory but they are both indicated as “well correlated”.

Agreed, we will amend our statement in the text, as per our response to reviewer 1 above. _____

3 L 23 I don’t understand what is meant by “Core issues surrounding the remoteness of satellite sensors”

Here we meant to identify that one of the limitations of satellite remote sensors is their remoteness from the ecosystems they measure. We will re-phrase the sentence to

BGD

Interactive
comment

Printer-friendly version

Discussion paper



state this more clearly. _____

3 L23-25 this sentence is rather obscure (“the diffuse nature of light”?). I would suggest to omit it and only mention that the highest temporal frequency available is one composite every 8-16 days.

We will omit the section identified by the reviewer so it reads more concisely.

3 L 34 I don’t understand “via leaf emergence and senescence”. Please rephrase.

This sentence is talking about the value of phenocams for identifying leaf-level changes, such as leaf emergence and senescence, so we will rephrase the sentence to more clearly show this. In particular, we will remove the word “via” as this seems to be the most misleading part of the sentence.

4 L1-3 Here you are saying that LUE models describes GPP through the relation between APAR and LUE. There is no relation, they are both used to estimate GPP.

Here we used the word ‘relation’ to indicate that the two parameters were multiplied to obtain GPP. We will reword this sentence to be clearer about this.

Section 2.3 The final field of view of the camera could be provided.

We will provide this in the section identified. _____

Section 2.3 Can you comment on possible effects of the automatic (and variable) white balance? This can variable from measure to measure. What is the effect on calculated indexes? Few numerical simulations may help in this assessment.

We do discuss, albeit briefly, the effects of white balance on image collection in the limitations section of our manuscript. The reviewer is correct in their assessment that white balance can vary from image to image, which is particularly more preva-

[Printer-friendly version](#)[Discussion paper](#)

lent during lower sun angles i.e. dawn/dusk. By using middle of day values, the effects of white balance can be reduced. However, white balance was set to zero in our analysis, which is a limitation in that it increases the scene illumination noise in our images. However, given that we only analysed middle of day images in an environment that is highly dynamic, the phenology signal was still identifiable. This may not be the case for a less dynamic ecosystem. Migliavacca et al. (2011) discuss the uncertainty and limitations of using digital camera imagery, which we make reference to in section 3.4. However, we will provide some additional discussion around this point in section 3.4 in order to address this comment by the reviewer.

8 eq 16-18 Why is the reflected PAR is not used? This is fIPAR. And the resulting flux is IPAR not APAR

Our answer to reviewer 1 about this should help to clarify this point.

10 L 34 In which sense “predictive” is used here? Is there any validation / prediction on independent data (i.e. not used in fitting)?

The relative ‘predictive’ error indicator we used in our analysis is simply a calculation of the % mean difference between two datasets. It provides an indication of the direction of change in the predicted values relative to the measured values in a relative sense. See Kanniah et al. (2009) Appendix 1 for further explanation and formulas for calculation.

Section 3.1 It would be interesting to see the FAPAR curves along with that of the various camera-indexes

This is a nice idea, but in the interest of balancing the additional information requests and the length of the manuscript in its current form, we think that creating and discussing an additional plot would make the manuscript un-

Printer-friendly version

Discussion paper



wieldly. However, we are happy to add these at the editors discretion and suggest that if the editor advises them, we include them as supplementary information.

Figure 7. Sorry, I am not getting what the 1:1 line refers to. The two variables on the scatterplot have different units and ranges

We can see why this would be confusing, it was meant to simply provide a guide of the deviation of the data, so we will remove the line from the resubmitted version.

Technical corrections:

3 L 4 Why “cover”?

We can remove the word ‘cover’ in the sentence to simply read “phenological change”

7 L 5-7 This sentence says that it is homogeneous and it is not. It’s a matter of scale. It can be rephrased.

This is absolutely true, but the sentence in question does state this: “While the understory is largely homogenous in species distribution at the flux tower scale (i.e. >50 m), variation from one point to another does exist in the understory due to its vegetation composition.” The sentence could be amended to read “. . .variation does exists at the smaller scale (i.e. < 5 m) in the understory due to. . .” to be a bit clearer on the subject.

8 L 13 “Absorbed” instead of “used”.

We will fix this in the revised version. _____

11 L9 RCC/ExR looks like a ratio. I would suggest to use “and”.

We will fix this in the revised version. _____

BGD

Interactive
comment

Printer-friendly version

Discussion paper



13 L1 I miss the integration in this section. The title of this section could be “Relation between GPP and time series of phenocam and MODIS indexes”

This section is about using the phenology information to improve estimates of GPP. Given this, we agree with the reviewer that the heading is misleading, therefore we propose to change it to “Using phenocam and MODIS phenology to improve GPP model estimates”. _____

P14 L5-7 Probably not needed, already described.

We will remove this sentence from the text.

Reviewer #3

The utility of phenology information for improving GPP modeling results is an important research objective and I find the present work interesting and relevant. The paper is well written, methods are sound and results are carefully discussed. However, descriptions are generally very (too) detailed and several sections would benefit from a slightly more concise format. The structure of parts of the methods section should also be improved for improved overview, flow and clarity.

We are pleased the reviewer enjoyed our manuscript and do agree that it is quite lengthy in parts. Given we used a rather home-made camera for our phenocams, we felt we should provide more detail about our methods. However, we will revise our manuscript and remove less-relevant information where possible. Alternatively, we can formulate a supplementary materials file that documents the camera setup in finer detail, and provide only a brief overview in the methods section of the manuscript.

Some detailed and relatively minor comments:

1. Page 1 L32: An R^2 of 0.09 – 0.23 does not constitute a well correlated relationship

Printer-friendly version

Discussion paper



as I see it.

This was also identified by reviewers 1 & 2, so we will fix this in the text as per our response to reviewer 1. _____

2. Page 2 L16: I believe fire should be capitalized as in “..2015). Fire: : :”

Yes it should, we will fix this in the revised manuscript.

3. Page 3 L19: What does the A2/A3 refer to? Is this information needed here?

The A2/A3 information refers to the sub-product of MOD17 used, as it is a combination of both GPP (A2) and NPP (A3) obtained from the Terra satellite. Given we only used the MOD17 A2 (i.e. GPP) product, we can omit the A3 reference, but feel the A2 reference should be kept for clarity. _____

4. Page 3 L20: MOD17 is mentioned to provide the most reliable means of estimating large-scale productivity. In comparison to what other products/estimates? MOD17 is known to be associated with significant uncertainty (related predominantly to the specification of the effective LUE), and I'm not convinced it will outperform other products given a full suite intercomparison.

We agree with the reviewer here in that there are a suite of GPP model products available. However, it is out of the scope of our study to compare all products. This sentence should therefore be amended to remove the “most reliable” portion with something reading “. . .the MODIS GPP product is widely-used means of estimating. . .” instead.

5. Page 3 L23: “Core issues surrounding: : :”; Odd sentence. Suggest rewording. The full sentence structure (L23 to L28) should be rewritten for better language and clarity.

This statement was also identified by reviewer 2, so we will fix the sentence based on our response provided previously. _____

6. Section 2 introduction (Page 4): This intro piece doesn't outline the overall methodology well and/or the sub-division of the methods sections. I would probably leave it out completely or provide a more elaborate and cohesive piece.

The intention of this short section was to provide a brief overview/blurb of the methods before describing what was done. Given the reviewers all commented on the length of our manuscript, we will omit it in the resubmission.

7. Page 6 L2: I don't think that it is necessary to know the type of coding language (Python) used..

We will remove this from the section identified.

8. Page 6 L31: "f/stop"?

This is a photography term that refers to the ratio of a lens' focal length to the diameter of the point where light enters the camera. It can be referred to as a focal point. We didn't feel it was necessary to describe it but could add "(focal point)" after it in the text.

9. Sections 2.3 and 2.4: The methods are described in great detail. I would suggest reducing the wordiness as much as possible only including the most essential elements.

We will endeavour to reduce the methods section as much as possible without losing the necessary information required to repeat the science. It is lengthy, but the paper also includes a range of measurement that need to be properly described.

While this reviewer has suggested we shorten the methods, the other two reviewers have encouraged us to provide more detail on some aspects of analysis (i.e. EC measurements, phenocam processing).

Eddy covariance in itself should really have a more detailed explanation than the one

Printer-friendly version

Discussion paper



given in our manuscript. Likewise, given the phenocams we used were a modified point-and-shoot digital camera, we needed to provide enough information about how they were set up and used in order for scientific replication. Similarly for the PAR, LAI and LUE model data. _____

10. Section 2.4: I would include separate sub-sections for the phenocam and radiation data processing for improved flow and readability. Line 13 on page 8 could be the start of the LUE sub-section.

This is a great suggestion and we will split the section where indicated by the reviewer.

11. Page 7 L24-26: I feel that this information is redundant.

We will remove this information in the re-submitted manuscript.

12. Page 8 L22: Shouldn't leaf absorptance be considered in the APAR calculation? You are using fPAR and not fAPAR, right?

Our response to reviewer 1 regarding this should help clarify this point.

13. Page 8 L24-: The information on LAI collection, clumping etc is out of place. You will need a separate section on this.

We will also separate this section into a new subsection in the methods.

14. Page 10 L1-4: Is it valid to adopt the default MOD17 savanna values for your study site? Did you verify these against the tower observations?

The Tmin and VPD values were previously validated for the Howard Springs site by Kanniah et al. (2009). However, we found slightly higher maximum VPD for our study period than that of Kanniah et al. (2009). Therefore, we cited the orig-

[Printer-friendly version](#)

[Discussion paper](#)



inal values of Running & Zhao (2015) for our study. Given Kanniah et al. (2009) did perform a validation of earlier values of Running et al. (2006) for savannas, we will include Kanniah et al. (2009) in our citation of the section identified.

15. Section 3.1 is very detailed and would benefit from a more concise format, if possible.

We will endeavour to shorten this section where possible in the resubmission.

References Cited:

BERINGER, J., MCHUGH, I., HUTLEY, L. B., ISAAC, P. & KLJUN, N. 2016. Dynamic INtegrated Gap-filling and partitioning for OzFlux (DINGO). *Biogeosciences Discuss.*, 2016, 1-36. KAIMAL, J. C. & FINNIGAN, J. J. 1994. Atmospheric boundary layer flows: their structure and measurement, New York, Oxford University Press. KANNIAH, K. D., BERINGER, J., HUTLEY, L. B., TAPPER, N. J. & ZHU, X. 2009. Evaluation of Collections 4 and 5 of the MODIS Gross Primary Productivity product and algorithm improvement at a tropical savanna site in northern Australia. *Remote Sensing of Environment*, 113, 1808-1822. MA, X., HUETE, A., YU, Q., RESTREPO-COUBE, N., BERINGER, J., HUTLEY, L. B., KANNIAH, K. D., CLEVERLY, J. & EAMUS, D. 2014. Parameterization of an ecosystem light-use-efficiency model for predicting savanna GPP using MODIS EVI. *Remote Sensing of Environment*, 154, 253-271. MIGLIAVACCA, M., GALVAGNO, M., CREMONESE, E., ROSSINI, M., MERONI, M., SONNENTAG, O., COGLIATI, S., MANCA, G., DIOTRI, F., Busetto, L., CESCATTI, A., COLOMBO, R., FAVA, F., MORRA DI CELLA, U., PARI, E., SINISCALCO, C. & RICHARDSON, A. D. 2011. Using digital repeat photography and eddy covariance data to model grassland phenology and photosynthetic CO₂ uptake. *Agricultural and Forest Meteorology*, 151, 1325-1337. MOORE, C. E., BERINGER, J., EVANS, B., HUTLEY, L. B., MCHUGH, I. & TAPPER, N. J. 2016. The contribution of trees and grasses to productivity of an Aus-

Printer-friendly version

Discussion paper



tralian tropical savanna. *Biogeosciences*, 13, 2387-2403. WHITLEY, R., BERINGER, J., HUTLEY, L. B., ABRAMOWITZ, G., DE KAUWE, M. G., EVANS, B., HAVERD, V., LI, L., MOORE, C., RYU, Y., SCHEITER, S., SCHYMANSKI, S. J., SMITH, B., WANG, Y. P., WILLIAMS, M. & YU, Q. 2016. Challenges and opportunities in modelling savanna ecosystems. *Biogeosciences Discuss.*, 2016, 1-44. WHITLEY, R. J., MACINNIS-NG, C. M. O., HUTLEY, L. B., BERINGER, J., ZEPPEL, M., WILLIAMS, M., TAYLOR, D. & EAMUS, D. 2011. Is productivity of mesic savannas light limited or water limited? Results of a simulation study. *Global Change Biology*, 17, 3130-3149.

[Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-187, 2016.](#)

BGD

[Interactive
comment](#)

[Printer-friendly version](#)

[Discussion paper](#)

