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Interactive comment on "A biophysical approach using drought stress factor for daily estimations of evapotranspiration and CO_2 uptake in high-energy water-limited environments" *by* David Helman et al.

Anonymous Referee #2

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The present sudy compares estimates of GPP and ET based on remotely sensed vegetation greenness (NDVI) and a light use efficiency (LUE=RUE) model with eddy-covariance (EC) measurements taken at six sites in a Mediterranean climates. Authors find that the inclusion of a drought stress factor (fDS) that downscales modelled ET and GPP based on the cumulative precipitation deficit (2 months) improves the agreement between EC measurements and the model. Authors further find that the modelled water use efficiency (WUE = GPP/ET) increases upon afforestation.

Global GPP estimates based on remotely sensed vegetation greenness (RS-models)

are widely used and their limitations under dry conditions have repeatedly been pointed out (Turner et al., 2005; Verstraeten et al., 2006; Maselli et al., 2009; Leuning et al., 2005; Mu et al., 2007; Pan et al., 2006). In all of these studies, the functional form of the relationship between GPP and water availbility (different indeces or soil moisture formulations used) is specified a priori (Verstraeten et al., 2006; Maselli et al., 2009; Pan et al., 2006; Leuning et al., 2005; Yuan et al., 2007) and its power for improving RS-based GPP estimates is evaluated within a specific modelling framework and for a limited number of sites. The present study adds to this body of literature with a special focus on sites in Israel, located in a Mediterranean climate (at the dry end of it). These ecosystems hence experience a pronounced dry season during summer months, where radiation is high but water availability low, possibly limiting GPP and ET. Hence, the finding that the inclusion of fDS is indeed important to accurately model fluxes is not surprising, but the study is a valuable contribution and underlines the shortcomings of models (e.g. MODIS-GPP) that do not account for direct effects of water availability.

Below, I am listing a few major points that I suggest need careful addressing before the study can be published. Remaining points are listed further below.

MAJOR POINTS

* All findings with regards to the importance of the drought stress factor are subject to the specific modelling framework applied here. This assumes that LUE is constant across the full range of vapour pressure deficit and light conditions and that its sensitivity to temperature is accurately captured (Eqs. 1 and 16). Furthermore, it assumes that the fraction of absorbed photosynthetically active radiation (fAPAR) is accurately captured by the function of NDVI used here (missing description of this function!). The modelling framework is simply an adoption of the model used by Maselli et al. (2009) and the specification of key parameters, although referring to Maselli et al., are arbitrary. Unfortunately, authors did not make any attempt to find calibrated parameter values, valid for their sites, test to what degree their conclusions are subject to these

choices, to try alternatives, or to discuss the caveats of this limitation. Of course, other comparable studies (Verstraeten et al., 2006; Maselli et al., 2009; Pan et al., 2006; Leuning et al., 2005; Yuan et al., 2007) are subject to the same limitation. But in some cases, I am concerned whether conclusions drawn here are valid (see next points).

* Based on their model results, it is concluded that WUE increases upon afforestation. In my understanding, the only difference that goes into estimating GPP and ET at the paired sites is NDVI (with temperature and water availability being identical at paired sites). The sensitivies of modelled GPP and ET to NDVI are different. Thus, WUE changes simply as a result of this differences, i.e. the difference in the derivative of GPP and ET to NDVI. Hence, the WUE changes found here are merely a model result. If my reading is right, I suggest to remove the respective statement from conclusions (l.668) and the abstract (l.47). (Why are WUE values provided here not based on actual measurements?)

* In a similar sense, the finding on l.605 ("In general, while using the drought stress factor did not improve (...) or only marginally improved (...) RS-Met estimates in the non-forest sites, it significantly improved the ET and GPP estimates in forest sites (...)") is contingent on the difference in NDVI between paired forest and non-forest sites and the sensitivity of ET and GPP to NDVI. Furthermore, here, no effects of vapour pressure deficity (VPD) on GPP and ET are considered, although evidence that stomatal conductance is sensitive to VPD is clear and this effect is accounted for in global vegetation models. Interpretating model-data agreement only in the light of the drought stress factor may thus be misleading.

The following general points are not as fundamental but I highly recommend addressing them.

* The scientific question to be addressed here (impact of water availability on ecosystem fluxes) is not clear and the relevance for the scientific community is not stated clearly. I recommend better work out the merits of the present study that go beyond

simply applying the method by Maselli et al. in another ecosystem. For this, it must also be stated more clearly what the generality of the findings are and to what degree they are limited by the scope (climate, ecosystem type). For example, could a global RS-based model perform well when combining it with the drought stress factor? (Compare l.673: "...represents a powerful basis for the reliable extension of ET and GPP estimates across spatial and temporal scales." and in abstract "This simple but yet robust biophysical approach show a promise for reliable ecosystem-level estimations of ET and CO2 uptake in extreme high-energy water-limited environments.") But even after reading the discussion, it is not clear why findings are only valid in extreme high-energy water-limited environments. What aspects of the method applied here are specific for such environments?

* Related to above point: In the introduction, the reference to the FAO-56 model looks very specific and not of particular interest to readers of Biogeosciences. Can this type of model be generalised? What information is used? And what other RS-based models are being used that correspond to this in structure? Is the FAO-56 model an analogue of the RUE model described in the subsequent paragraph (from l. 105)? Resolving these points would help to have the problem at hand here appear more general.

* The use of data from different sites is confusing. For some, continuous flux measurements are available (Yatir), but also apparently at Eshtaol [Fig. 2], althtough in my reading, only the mobile EC measurement device was used at Eshtaol. One may ask if the few time points of measurements at the sites with the mobile EC device and the very high scatter (Fig. 6) even justify the use of this data. Here, I recommend stating that measurements at "mobile" sites were taken during wet and dry periods of the year. Then show that during wet periods, agreement between EC and RS-met is ok, but not during dry.

* Why was there no cross-validation of the mobile EC measurement device with the fixed installed flux tower?

SPECIFIC POINTS

* I suggest not to state that the PaVI-E model is used for validation. Validation should always be against data.

* Methods: NDVI of soil and vegetation: Where are values taken from and why is local vegetation varying just within these?

* Methods: Eq. 2-7 could be avoided (except Eq. 5) and just start with Eq. 8.

* Absolutely a must: showing fAPAR function of NDVI

* I got confused by the use of names for models (DS model [l. 422], RS-met model, WS model [l. 587], etc.)

* In Eq. 13, I suggest to replace fPAR with fAPAR in order to avoid confusion (fPAR may also mean the fraction of photosynthetically active radiation, not necessarily absorbed).

* Some data inputs are intransparent: What are the actual values of NDVI at forest and non-forest sites? What is fAPAR during the season (show it in Fig. 2)?

* I cannot subscribe to a number of statements made in the introduction:

* "Estimations of ecosystem-level evapotranspiration (ET) and CO2 uptake in water-limited environments are scarce" and "most EC towers are concentrated in the US, Europe and Asia, with poor coverage in water-limited regions". The FLUXNET 2015 dataset includes numerous stations in dry ecosystems, e.g. in Australia and the Southwest US.

* l. 82-84: Unclear what "too complex" means. Just regarding the accessibility and usability, or too complex model formulation? And what is "too complex" and what isn't? Kalma et al., 2008 treat only RS-based ET models. The RS-based GPP model (MTE-GPP) by Jung et al. (2011) is widely used by in the carbon cycle community.

* l. 136: Ahlstroem et al. refer to semi-arid regions in general, not "this regions" as in

Israel, or Yatir forest, which the formulation implies.

ABSTRACT

* "biophysical approach was previously proposed": This description is too generic to provide the necessary information needed to understand what is being done here.

* "RS-met": add the word 'remote sensing' somehow in order to provide a comprehensible description of what RS-Met means.

* "ETMOD =0.94×ETEC + 0.28": Too many abbreviations that are not introduced and numbers which are unclear what they mean.

INTRODUCTION

* "utmost": tone down.

* Starting the introduction with introducing tree ring data and isotopes might be a bit off the main scope of the paper

* l. 89: References Glenn et al., 2010, deal only with RS-based ET models, but not GPP.

* l. 110: ("fPAR") This should be the fraction of *absorbed* PAR.

METHODS

* Missing from description (but relevant for the questions at hand here):

* - available water capacity of the soil

* - soil texture

* - soil drainage

* - groundwater table depth

* l. 264: Smoothing can be problematic: it removes also real seasonal peak and

troughs with implications for the GPP (and thus fDS). How is this addressed?

* l. 285 (“conventional”): There is actually some disagreement to this “convention” (see Weir et al., 2016 Nature). Use a different wording.

* Choices for NDVIsoil and NDVIveg are not clearly stated and lack a reference. How come, observed NDVI never exceeds or goes below these values?

* l. 406: Is this the two months preceeding the day of measurement?

* l. 413: (NDVI - NDVIsoil) / fVC instead of NDVI/fVC?

RESULTS

* l. 554: spell out which site.

* l. 567: are these particularly dry years?

* l. 583: If ET from RS-met is higher than P, then the most obvious implication is that the fDS factor is not sufficiently responsive to low wate availability. In case of EC, could there also be an issue with energy balance closure in EC measurements?

* l. 611: this formulation is a bit exaggerated (“tracked seasonality”). it's basically two point measurements during one year, or am I getting something wrong?

CONCLUSIONS

* l. 654 “models”: plural justified here?

FIGURES

* Fig. 2: x-axis: J is not unambiguous, A neither. write at leas Apr, Aug, Jan and Jul. add years of measurements as well.

* Fig. 3: missing legend for grey vs black lines and points in inserts

* Fig. 6: include the no-DS time series in plots

* Fig. 7: “across six sites”: unclear what data is going in there: from six sites where mobile device was used for measurements, no continuous measurements are available. What does this represent here then?

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