Responses to Anonymous Referee #2 comments:

Thank you for your thoughtful comments. Including your suggested revisions has improved the quality of the manuscript. Our responses are indicated below in blue text.

The present study 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 climate. Authors find that the inclusion of a drought stress factor (f_{DS}) 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 availability (different indices 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 f_{DS} 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.

Au: We thank you for the positive assessment of our manuscript (MS).

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

1. 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).

Au: We did not use only temperature in the modelling of LUE but also radiation and water supply (rain) data; thus we believe we do capture quite well seasonal changes in LUE. It is true that using VPD could have improved our model; however, we intend to provide in this study a simple model that uses minimal sets of input data that can be easily downloaded as available satellite products and basic meteorological information from standard weather stations. Information on VPD is rarely available from weather stations in our region and would limit the application of the RS-Met to the few stations with such information. In addition, the f_{WD}, which is based on radiation, temperature and rainfall data serves in the RS-Met as a proxy for VPD conditions. We added this to the text:

"Importantly, these MODIS products take advantage of the use of vapor pressure information, which was shown to affect the stomatal conductance of plants whereas our model did not consider this factor directly. We did not use vapor pressure data in the RS-Met because most of the weather stations in this region do not have such information and that would have limited the use of our model. However, the f_{WD} calculated from radiation, temperature and water supply (rainfall) data, is used in the adjusted RS-Met as an indirect proxy for VPD."

2. 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!).

Au: We used here the fAPAR formulation of Myneni & Williams (1994), which showed good results when applied in Monteith-type GPP models in different ecosystems and under diverse environmental conditions. We have no means to assess here the validity of the formulation; however, we do believe that it does work well in our sites (see Helman et al. 2017) though further testing should be conducted on this in the future. We added the formulation of the fAPAR in the revised MS:

"The fAPAR was derived here from the daily NDVI time series following the linear formulation: fAPAR = 1.1638 NDVI – 0.1426, which was proposed by Myneni & Williams (1994). This linear formulation was successfully applied in similar remote-sensing-based GPP models for similar ecosystems by Veroustraete et al. (2002), Maselli et al. (2006, 2009) and Helman et al. (2017)…"

3. 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).

Au: We acknowledge that the model could be improved through calibrating the parameter values at the site level. However, the strength of the model in its present form is in its consistency of the used parameters that allows its use not only locally but also at a wider spatial scale and in distinct ecosystems under different environmental and water supply conditions.

4. 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 sensitives 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?)

Au: This is not accurate. Temperature and radiation were not identical in paired sites (though we did use the same rainfall data for the paired sites). Thus, WUE changes were a result of the difference in ETo, PAR, f_{WD}, T_{corr} and not only NDVI between paired sites.

5. 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.

Au: Please see our response to the previous comment.

6. Furthermore, here, no effects of vapour pressure deficit (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. Interpreting model-data agreement only in the light of the drought stress factor may thus be misleading.

Au: We acknowledges the importance of VPD on GPP and ET fluxes; however, as previously stated, we intend to provide in this study a simple model that uses minimal sets of input data that can be easily downloaded as available satellite products and basic meteorological information from standard weather stations. Information on VPD is rarely available from weather stations in our region and would limit the application of the RS-Met to the few stations with such information. Also, the f_{WD}, which is based on radiation, temperature and rainfall data serves in the RS-Met as a proxy for VPD conditions. Please see our response the comment #1.

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

7. 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 CO_2 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?

Au: We have revised the MS according to this suggestion. Please see changes in the Discussion, Introduction and along the MS.

8. 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.

Au: The FAO-56 formulation is the core of the RS-Met model of ET. Indeed, it is in that sense the analogue of the RUE model. We added short explanation on the original FAO-56 formulation and referred to Eq. 2 (which is a two coefficient version of the original FAO-56 formulation).

"One of those models is the ET model based on the FAO-56 formulation (Allen et al., 1998). The FAO-56 formulation states that the actual ET of irrigated crops can be determined from the reference ET (ET_o) corrected with crop coefficient Kc values (see Eq. 2). The Kc varies mainly with specific plant species characteristics, which enables the transfer of standard Kc values among locations and environments (Allen et al., 2006).

The remote-sensing version of this formulation, uses a function of satellite-derived vegetation index, usually the normalized difference vegetation index (NDVI), as a substitute for the crop coefficient...."

9. The use of data from different sites is confusing. For some, continuous flux measurements are available (Yatir), but also apparently at Eshtaol [Fig. 2], although 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.

Au: We have changed former **Fig. 2**, partly due to this confusion (see revised **Fig. 2**). Now it shows the model, model drivers and flux observations in Yatir (the only site with continuous flux measurements). We also refer to dry and wet seasons in the text (see response to comment #37). As already pointed out in the Introduction, there are almost no flux measurements in this regions (in vegetated systems) so every flux data available is really appreciated. We decide to show the comparison between modelled and observed fluxes during the dry season because it remarks the important role of the f_{WD} in reducing the *fVC*, ETo, RUE and PAR in the model during this period.

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

Au: A cross-validation of the mobile EC measurement device with the fixed installed flux tower at Yatir was conducted with measurements from April 2012. We added this information in the revised MS:

"During April 2012 at the peak activity season in Yatir forest, the mobile lab system for two weeks deployed at 10 m distance away from the permanent flux measurements tower, were both EC systems measuring at the same height and fluxes calculated by the same software (EddyPro 3.0 version; Li-Cor, USA). The linear correlation (R^2) and the slope of the Mobile Lab measured fluxes of H, LE and NEE vs. the permanent Tower fluxes were 0.9 and 1.0 for H, 0.8 and 0.9 for LE and 0.9 and 1 for NEE, respectively."

SPECIFIC POINTS

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

Au: Changed to:

"We also <u>compared</u> the RS-Met ET estimates to the annual ET derived from PaVI-E (Parameterization of Vegetation Index for the estimation of ET model; Helman et al., 2015a), at the six sites on an annual basis…"

12. Methods: NDVI of soil and vegetation. From where these values were taken from and why is local vegetation varying just within these values?

Au: This is the observed range in NDVI over bare ground and natural ecosystems in this region:

"We used here the values of 0.1 and 0.8 for the $NDVI_{SOIL}$ and $NDVI_{VEG}$, respectively, which are the values observed for bare ground and dense natural vegetation in this region (Helman et al., 2015b)."

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

Au: We think that Eqs. 2-7 should be presented because they are used to explain the FAO56 formulation (Eq. 2; see response to comment #7), which is the basis of the ET model, and the RS adjustments to this formulation (Eq. 3-7). Starting with Eq. 8 would be too confusing for readers not familiar with the RS-based FAO56 approach. Thus, we decide to leave Eqs. 2-7 in the revised version of our MS.

14. Absolutely a must: showing fAPAR function of NDVI

Au: The formulation of the fAPAR was added in the revised MS:

"The fAPAR was derived here from the daily NDVI time series following the linear formulation: fAPAR = 1.1638 NDVI – 0.1426, which was proposed by Myneni & Williams (1994)."

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

Au: We change it to simply "the model with and without the f_{WD}" along the MS and in the revised **Fig. 3** to "(f_{WD})" and "(NO f_{WD})", respectively (please see **Fig. 3e,f** below).

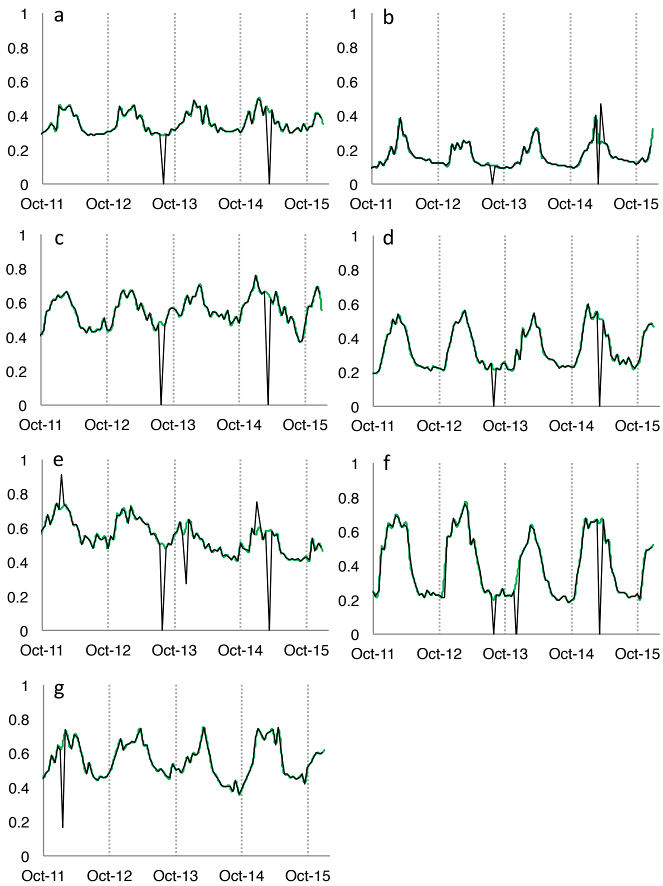
16. In Eq. 13, I suggest to replace fPAR with fAPAR in order to avoid confusion (fPAR may also mean the fraction of photosynthetically active radiation, not necessarily absorbed).

Au: Changed to *f*APAR as suggested.

17. 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)?

Au: fAPAR is shown in **Fig. 2** (see revised figure in our response to comment #39 below). In addition, we have included time series of the original 16-day MODIS NDVI (MOD13Q1) in each site and the respective interpolated (smoothed) time series in **Fig. S2** in the Supplement of this MS:

Fig. S2:



18. 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 South-west US.

Au: This is true but there are only few flux stations in the mentioned regions and only one station in the extreme arid region of the EM (Please see Schimel et al., 2015).

19. 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.

Au: Changed to:

"Many RS models for the estimation of ET and GPP exist (see review in Kalma et al., 2008), but these algorithms are too complex and most of the models are not provided as accessible products for researchers outside the remote sensing community. Particular exceptions are the satellite-borne Moderate Resolution Imaging Spectroradiometer (MODIS)-based ET and GPP products (MOD16 and MOD17), which provide 8-day ET and GPP estimates at 1-km for 2000-2015, globally (Mu et al., 2007, 2011, Running et al., 2000, 2004)."

Also, Jung et al. (2011) is an excellent model but its spatial resolution is too coarse for local use (0.5 degrees).

20. l. 136: Ahlstroem et al. refer to semi-arid regions in general, not "this region" as in Israel, or Yatir forest, which the formulation implies.

Au: Changed to:

"Moreover, despite of the well-known important contribution of drylands regions to the global CO_2 (Ahlström et al., 2015), there are almost no efforts of estimating CO_2 fluxes in forested and non-forested areas in this dry region."

ABSTRACT

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

Au: We have revised the Abstract of this MS as follows:

"Abstract

Estimations of ecosystem-level evapotranspiration (ET) and CO_2 uptake in water-limited environments are scarce and scaling up ground-level measurements is not straightforward. A biophysical approach using remote sensing (RS) and meteorological data (RS-Met) is adjusted to extreme high-energy water-limited ecosystems that suffer from continuous stress conditions to provide daily estimations of ET and CO_2-uptake (measured as gross primary production – GPP) at a spatial resolution of 250-m. The RS-Met was adjusted using a seasonal water deficit factor (f_{WD}) based on daily rainfall, temperature and radiation data. We validated our adjusted RS-Met with eddy-covariance flux measurements using a newly developed mobile lab system and the single active Fluxnet station operating in this region (Yatir pine forest station) in a total of seven forest and non-forest sites across a climatic transect in Israel (280-770 mm y^{-1}). RS-Met was also compared to the satellite-borne Moderate Resolution Imaging Spectroradiometer (MODIS)-based ET and GPP products (MOD16 and MOD17, respectively) in these sites.

Results show that the inclusion of the f_{WD} significantly improved the model, with R=0.64-0.91 for the ET adjusted model (compared to 0.05-0.80 of the non-adjusted model) and R=0.72-0.92 for the adjusted GPP model (compared to R=0.56-0.90 of the non-adjusted model). The RS-Met (with the f_{WD}) successfully tracked observed changes in ET and GPP between dry and wet seasons across the sites. ET and GPP estimates from the adjusted RS-Met also agreed well with eddy covariance estimates at the annual timescale in the Fluxnet station of Yatir (266±61 vs. 257±58 mm y^{-1} and 765±112 vs. 748±124 gC m^{-2} y^{-1} for ET and GPP, respectively). Comparison with MODIS products showed consistently lower estimates from the MODIS-based models, particularly at the forest sites. Using the adjusted RS-Met, we show that afforestation significantly increased the water use efficiency (the ratio of carbon uptake to ET) in this region, with the positive effect decreasing when moving from dry to more humid environments, strengthening the importance of drylands afforestation. This simple but yet robust biophysical approach shows a promise for reliable ecosystem-level estimations of ET and CO_2 uptake in extreme high-energy water-limited environments."

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

Au: Please see response to the previous comment above.

23. "ET_{MOD} =0.94×ETEC + 0.28": Too many abbreviations that are not introduced and numbers which are unclear what they mean.

Au: We have deleted this information from the Abstract. Please see also our response to comment #21 above.

INTRODUCTION

24. "utmost": tone down.

Au: OK, changed to:

"Assessing the water use and carbon uptake in terrestrial ecosystems is important for monitoring biosphere responses to climate change (Ciais et al., 2005; Jung et al., 2010; Reichstein et al., 2013)."

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

Au: OK. Removed from the MS.

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

Au: Changed to:

"(i.e. daily estimates at 250 m; see e.g. Veroustraete et al., 2002; Sims et al., 2008; Maselli et al., 2009, 2014; and review of ET models in Glenn et al., 2010)."

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

Au: Changed to *f*APAR (here and along the MS).

METHODS

28. Missing from description (but relevant for the questions at hand here):
- available water capacity of the soil

- soil texture
- soil drainage
- groundwater table depth

Au: All available information on the studied sites is provided in *Section 2.1.*

29. l. 264: Smoothing can be problematic: it removes also real seasonal peak and troughs with implications for the GPP (and thus f_{DS}). How is this addressed?

Au: This is true. However, we kept important information in the time series by using LOESS with a narrow window as in Helman et al. (2014a,b; 2015b). We have added Fig. S2 in the Supplement of this MS showing original and smoothed NDVI time series (see Fig. S2 above in the response to comment #17). We also revised the text, accordingly:

"Then, we pre-processed the NDVI time series as described in Helman et al. (2014a, 2014b, 2015b) to remove outliers and uncertainties due to cloud contamination and atmospheric disturbances without removing important information (see Fig. S2)"

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

Au: Changed to:

"GPP for each site was calculated from the measured net ecosystem CO_2 exchange (NEE) after estimating ecosystem respiration, Re, and using the regression of NEE on turbulent nights against temperature, followed by extrapolating the derived night-time Re-temperature relationship to daytime periods (Reichstein et al., 2005; modified for our region by Afik, 2009)."

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

Au: Please see our response to comment #12 above.

32. l. 406: Is this the two months preceding the day of measurement?

Au: Yes.

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

Au: This lines were changed to:

"This reduction in the f_{WD} accounts for water deficit at the root zone, which results in reduced plant transpiration, while short-term effects would be mainly reflected through changes in the NDVI (and consequently in the fVC and fAPAR; Glenn et al., 2010; Running and Nemani, 1988)."

RESULTS

34. l. 554: spell out which site.

Au: These lines were changed and name of the site was added:

"We then examined the adjusted RS-Met on an annual scale, first by comparing the inter-annual variation in the modeled ET with that from the EC and with that from the MODIS ET product at Yatir (Fig. 5a)."

35. l. 567: are these particularly dry years?

Au: Yes. We added this in the text:

"The little year-to-year variation in the MODIS ET resulted in a noisier pattern of water use (green line in upper panel of Fig. 5a) compared to that calculated from the RS-Met and EC. A noisy water use pattern was also noted in the RS-Met (compared to that from the EC), particularly in dry years (Fig. S3; e.g., 2003, 2005 and 2008; Fig. 5a). Higher ET in the RS-Met was likely the result of discrepancies in daily estimates during the summer between the RS-Met and EC (R=0.05; P>0.1 for June-August; Fig. 5d). This is supported by the observation of a 5-fold higher bias between EC and RS-Met summer daily estimates in those dry years (bias = -0.146 mm d^{-1}), compared to remaining years (bias = -0.029 mm d^{-1})."

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

Au: The energy balance closure at Yatir is close to 1.0 (please see Rotenberg and Yakir, 2011). Thus, we are quite confident in our results. Higher ET than P in drylands ecosystems have been previously observed in this region and elsewhere (Helman et al., 2016; Raz-Yaseef et al., 2012; Williams et al., 2012), as pointed out in the MS, implying the use of water stored in deep soil layers during wet years (e.g., 2006 and 2008; Raz-Yaseef *et al.*, 2012; Barbeta *et al.*, 2015).

37. 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?

Au: Changed to:

"The adjusted RS-Met successfully tracked changes in ET and CO_2 fluxes from dry to wet season in all sites."

CONCLUSIONS

38. l. 654 "models": plural justified here?

Au: Changed to "model" or just "RS-Met" along the MS.

FIGURES

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

Au: We have changed and revised **Fig. 2** (now it shows the model, model parameters and observed fluxes in Yatir forest) and **Fig. 3e,f** and added years of measurements in the caption of revised **Fig. 2** for clarity, as suggested:

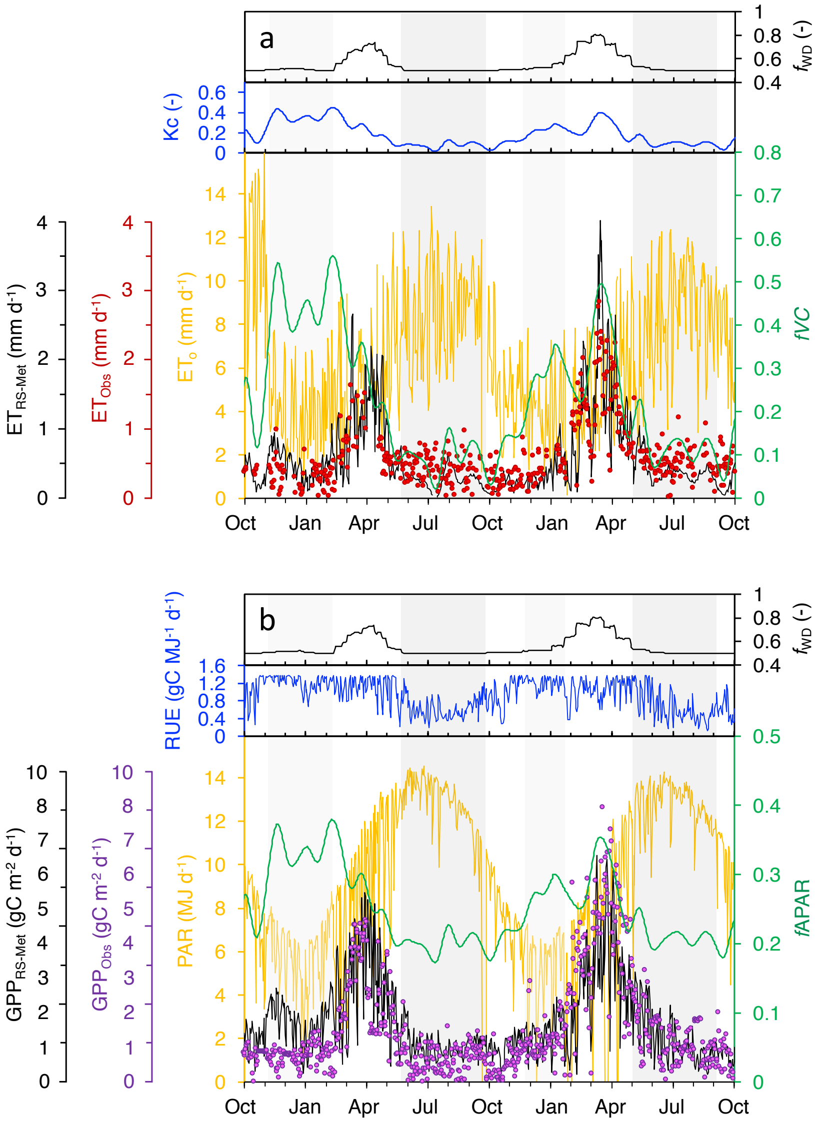
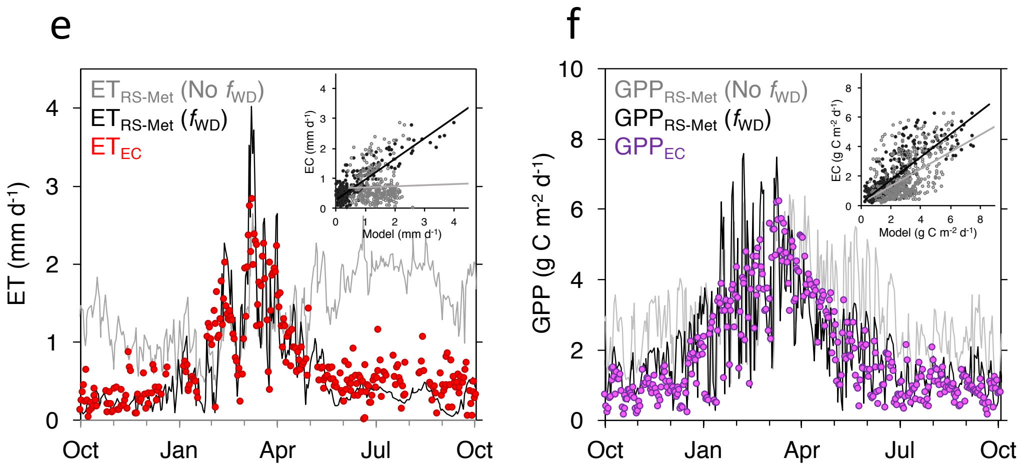


Fig. 2. Seasonal evolution of the water deficit factor (f_{WD}; black line in upper panel) and the main drivers of the modeled ET **(a)** and GPP **(b)** at the semiarid pine forest of Yatir ($ET_{RS\text{-}Met}$ and $GPP_{RS\text{-}Met}$, respectively; black line in lower panel) for the seasonal years 2008/9 and 2009/10. EC fluxes are also shown (ET_{Obs} and GPP_{Obs}, red and purple dots, respectively). The K_C and the radiation use efficiency (RUE) both without the addition of the f_{WD} (blue in middle panels) are shown together with the potential ET (ET_o; yellow in **a**), the fraction of vegetation cover (*fVC*; green in **a**), the photosynthetic active radiation (PAR; yellow in **b**), and the fraction of absorbed PAR (*f*APAR; green in **b**). Colored vertical bands indicate the critical periods when the addition of f_{WD} is particularly useful.



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

Au: **Fig. 3e** and **f** were revised (please see above).

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

Au: Estimates from the model without the f_{WD} were added in a new **Fig. S4** in the Supplement of this MS. We added also MODIS ET/GPP products (MOD16 and MOD17, respectively) in the revised **Fig. 7** as suggested by Referee #1.

Fig. S3:

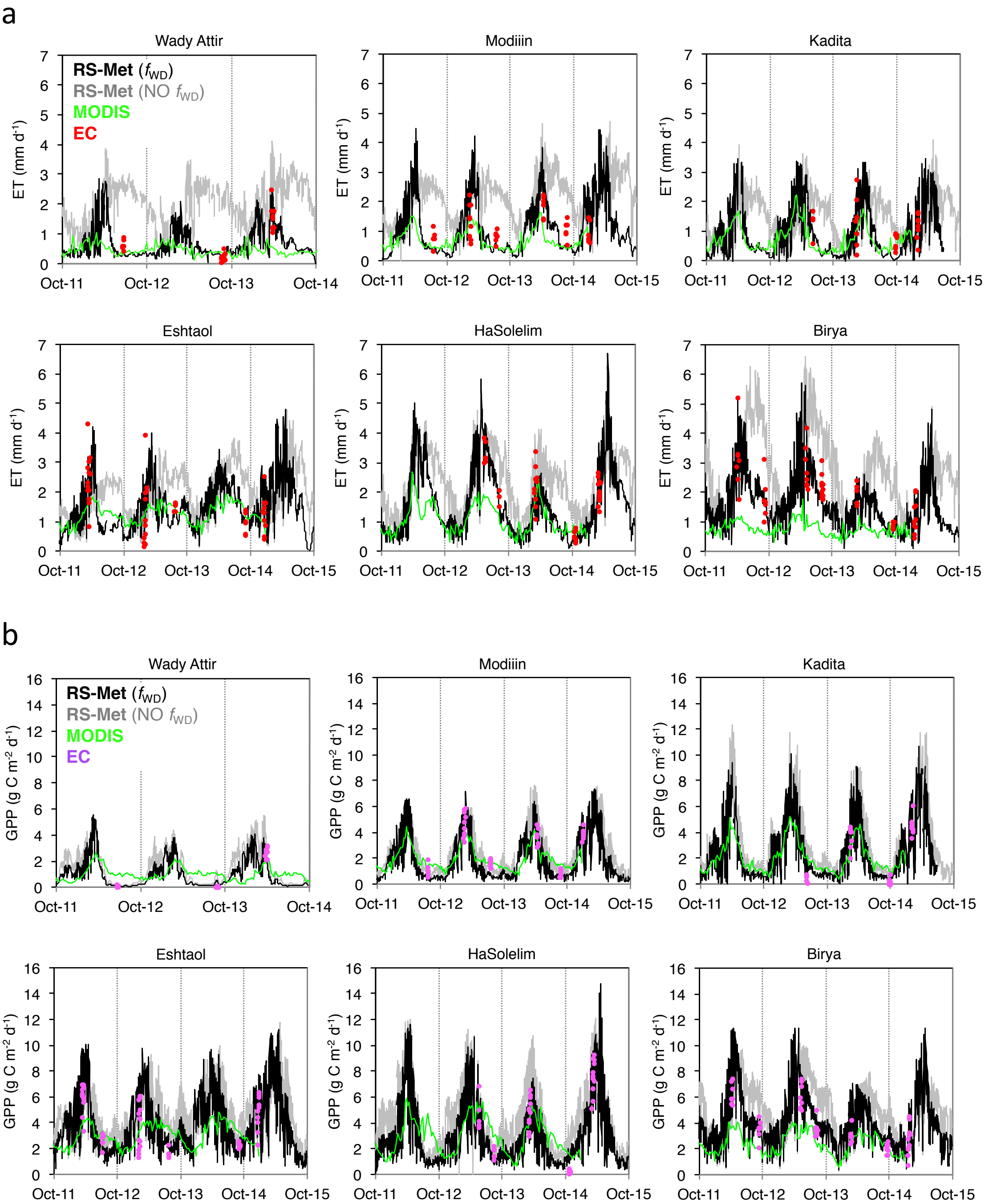


Fig. S4. Same as Fig. 7 in main article with the addition of RS-Met without the f_{WD} (grey line).

Fig. 7:

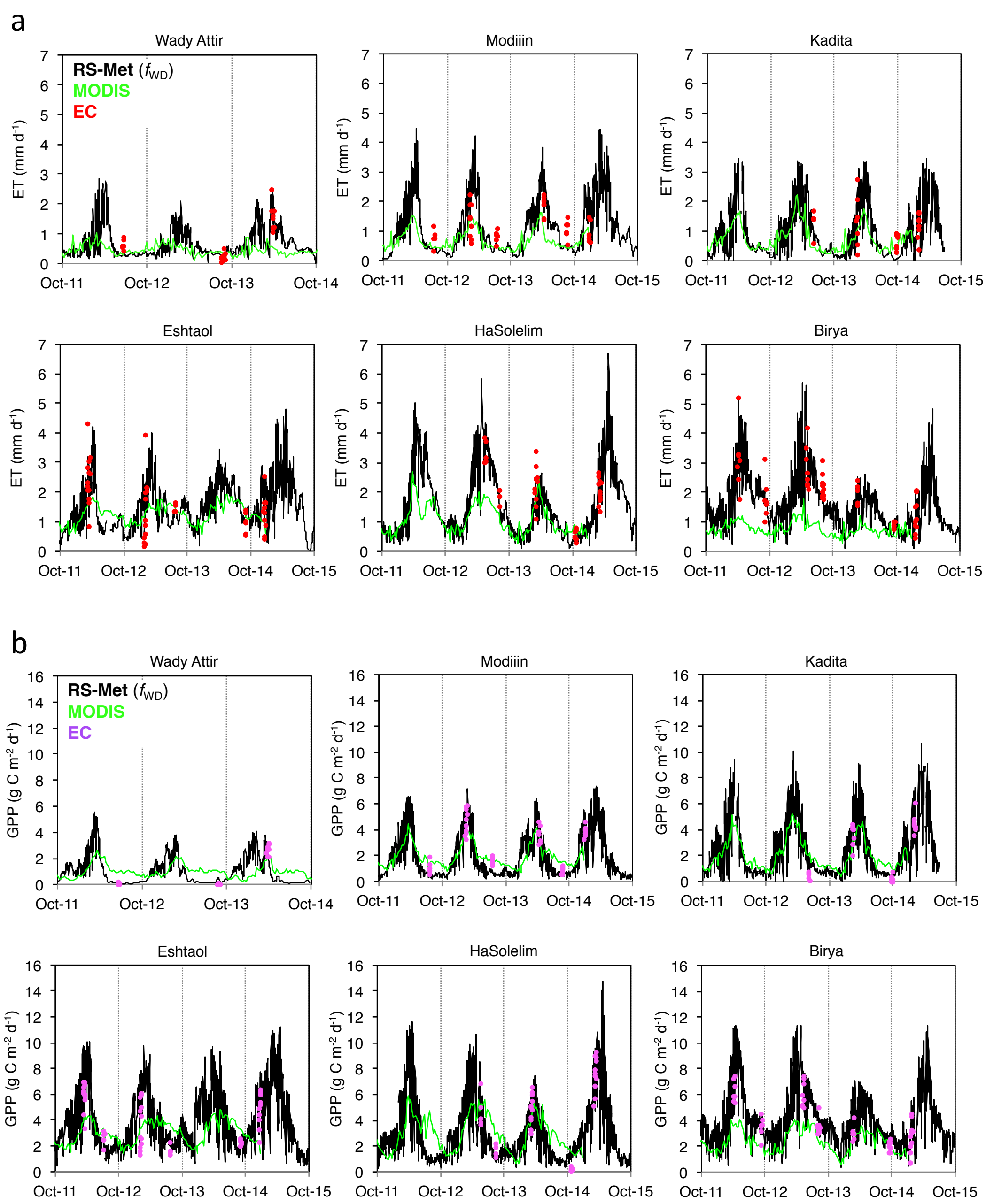


Fig. 7. ET **(a)** and GPP **(b)** from EC, RS-Met (with f_{WD}) and MODIS (MOD16A2 and MOD17A2 products, respectively) at the six forest and non-forested sites.

42. 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?

<u>Au:</u> This is a cross-site correlation between RS-Met fluxes vs. EC fluxes using all data available. We added this in the caption. We also have revised this figure.

"Fig. 8. Cross-site EC vs. model correlations of ET (a, c) and GPP (b, d). In (a) and (b) are the EC vs. RS-Met (with f_{WD}) using all EC data from the six sites (each dot representing a single date)…"

Fig. 8:

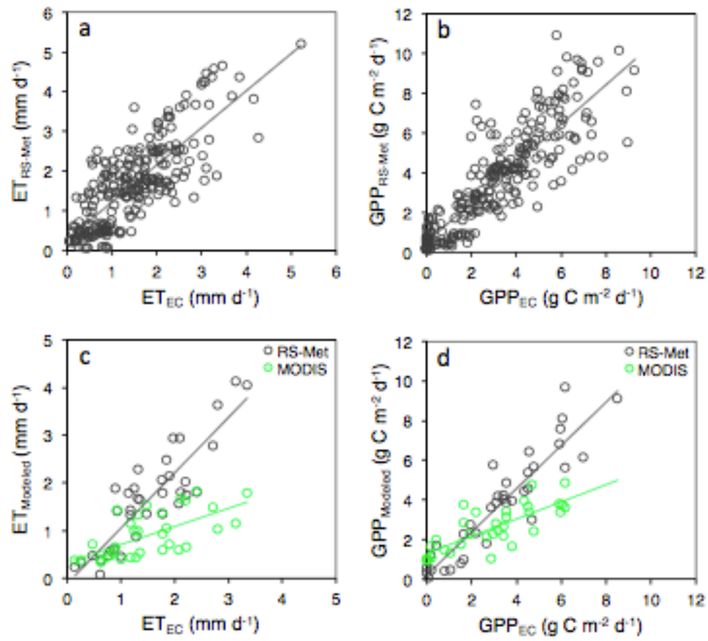


Fig. 8. Cross-site EC *vs.* model correlations of ET **(a, c)** and GPP **(b, d)**. In **(a)** and **(b)** are the EC *vs.* RS-Met (with f_{WD}) using all EC data from the six sites (each dot representing a single date), with linear fits of $ET_{RS\text{-}Met}$ = 0.936 ET_{EC} + 0.281 (R = 0.82; P<0.0001; N = 243) and $GPP_{RS\text{-}Met}$ = 0.990 GPP_{EC} + 0.515 (R = 0.86; P<0.0001; N = 252) for ET and GPP, respectively. In **(c)** and **(d)** are the same cross-site correlations but with data averaged over 8-day periods for comparisons with MODIS ET and GPP products (8-day averaged values). Linear fits for EC *vs.* RS-Met and MODIS in **(c)** are $ET_{RS\text{-}Met}$ = 1.16 ET_{EC} - 0.11 (R = 0.88; P<0.0001; N = 36) and ET_{MODIS} = 0.38 ET_{EC} + 0.33 (R = 0.65; P<0.0001; N = 33), respectively. In **(d)**, linear fits are $GPP_{RS\text{-}Met}$ = 1.09 GPP_{EC} + 0.21 (R = 0.92; P<0.0001; N = 36) and GPP_{MODIS} = 0.43 GPP_{EC} + 1.31 (R = 0.77; P<0.0001; N = 33) for EC *vs.* RS-Met and MODIS, respectively.

REFERENCES (DOI):

Turner et al., 2005: https://doi.org/10.1111/j.1365-2486.2005.00936.x
Verstraeten et al., 2006: https://doi.org/10.1016/j.ecolmodel.2006.06.008
Maselli et al., 2009: https://doi.org/10.1016/j.rse.2008.11.008
Leuning et al., 2005: http://dx.doi.org/10.1016/j.agrformet.2004.12.004
Mu et al., 2007: 10.1029/2006JG000179
Pan et al., 2006: 10.1890/05-0247
Yuan et al., 2007: http://dx.doi.org/10.1016/j.agrformet.2006.12.001