Response to comments by Reviewer #1 on "Environmental controls on seasonal ecosystem evapotranspiration/potential evapotranspiration ratio as determined by the global eddy flux measurements" *by* Chunwei Liu et al.

We greatly appreciate the review comments and thank the reviewers for their effort. We have addressed all of the comments and present our response below. The review comments are in regular typeface, while all responses are in italics and boldface.

The manuscript "Environmental controls on seasonal ecosystem evapotranspiration/ potential evapotranspiration ratio as determined by the global eddy flux measurements" by Liu et al. explores the possibility to extend the use of 'crop coefficients' from crops (as proposed by FAO) to natural vegetation. The manuscript also attempts to estimate such coefficients based on eddy covariance data from several locations in the world.
The idea is interesting, as potentially one could estimate the actual evapotranspiration from easy-to-obtain basic meteorological data, geographical location and vegetation type. Nevertheless, I think the manuscript does not deliver what it promises. The bulk of results focuses on the correlation between crop coefficient and climatological data or basic ecosystem properties (e.g., LAI), presenting mostly expected relations. The impact of this work would be greatly enhanced should the authors really tested their approach, by, e.g., calculating the crop coefficients on the basis of their multivariate linear model and basic ecosystem and climatic data and comparing the results with the estimates from eddy covariance data.

AUTHOR RESPONSE: Good suggestion. In the revision, we added new model validation results that examine the multivariate linear model using 30 other sites. The results show that the multiple models can be used for calculating monthly Kc, and monthly AET sufficiently at a large spatial scale and homogeneous ecosystems (Fig.8). (Line 130-133, 216-221, 285-290)

Aside from the specific results, the manuscript and methodology suffer from several, mostly addressable, issues: - Time scales are important, as some processes may be relevant at specific scales. Yet, it remains unclear throughout the manuscript at what time scales the method is applied and to which scales the data refer. Specifically: is the method applied at the annual time scale or at the monthly time scale? Are the data shown monthly (or annual) averages for a specific year or across several years?
AUTHOR RESPONSE: The time scale for Kc calculation is really important for model applications. Our method was applied at the monthly time scale. In the establishment of the multiple Kc models, we use monthly average Kc for several years in different sites. We only chose 78 sites to construct the model. Most validation sites have only 1-2 years eddy flux data, which do not represent the whole Kc variations among years.

To what time scales do the following statements refer? L 59 (subdaily to seasonal?), L 69 (decades to centuries?), L 88 (within a certain developmental stage?), L 138 (daily, monthly, annual or multi-annual means?)

AUTHOR RESPONSE: The time scale of AET/PET in L60 was annual. We have added this info in the manuscript. Sun et al (2015) focus on the monthly AET/PET (L68), and we made improvement in this study. The work of Kc mentioned in L88 was for growing seasons and the time scale was mostly daily. The calculation of ET_0 were calculated at the daily time scale, and we use a monthly total AET and PET to calculate Kc (L146) in this study.

Most of the eddy covariance sites are mid-to-high latitude sites, where most likely temperature and solar radiation are the limiting factors for evapotranspiration during part of the year, potentially even leading to leaf shedding in deciduous ecosystems or absence of crops in some cropping systems. Hence, rather than working at the annual scale (as suggested by L 173), it would be probably more meaningful to restrict the analyses to months in which vegetation indeed drives actual evapotranspiration, e.g., on the basis of LAI dynamics or an indicator based on temperature/day length. This would also mean considering dry/wet seasons in the few tropical ecosystems. More in general, this work would benefit from more attention to the main mechanisms defining actual and potential evapotranspiration. Accounting for seasonality is an example in this sense. Another example is the role of temperature, which appears not relevant in the introduction and method description, yet impacts both potential and actual evapotranspiration in a nonlinear way, directly and indirectly (e.g., via vegetation).

AUTHOR RESPONSE: The manuscript validates against AET using "crop coefficient method" with the eddy flux data in different land covers. However, the ratio of AET/PET is known as Kc in ET simulation for crops, and was influenced by ratio of soil evaporation to ET, canopy resistance, albedo of the canopy surface, and height of the crops in field scale (Allen et al, 1998, FAO 56). Thus, we try to calculate the AET/PET through the analyses on environmental factors including latitude, precipitation, leaf area index in a larger spatial scale. The factors such as temperature and solar radiation were used for PET calculation, and were not independent to AET/PET, as a result, we only chose independent factors to simulate the AET/PET. (Line 179-183)

The monthly AET/PET indeed can be affected by the dry/wet seasons, so we choose precipitation as an independent factor. The latitude is constant in the same site during all months, so we choose average annual AET/PET to analyze the response of AET/PET to latitude, which is a comprehensive factor for different plant communities for the same IGBP land cover type.

Finally, it would be helpful to have some more information on the crops – if annual summer crops, their winter Kc (L 164) represents other, non-vegetation related, mechanisms.

Yes，the winter seasons AET for CRO and OS is mainly depended on soil evaporation as the vegetation deforested or harvested. Thus, the different growing seasons for different crops may cause errors in modeling AET.

Finally, the dataset available to the authors is heavily dominated by temperate and boreal ecosystems, with very few tropical sites. I am well aware that only limited eddy covariance data are available from low-latitude sites. Nevertheless, I think that the authors should either limit their attention to temperate and boreal sites (underlining this limitation in their results) or obtain at least few more

datasets from the currently under-represented ecosystems/regions. This second approach may require moving beyond FLUXNET data, but may greatly enhance the impact of the work.

Yes，there are still two IGBP land cover types we don't include, which are savannas(SAV) and closed shrubland (CS). The initial dataset contains different number of year, thus, we only chose the monitoring data more than 2 years to establish the multiple AET/PET model. Most of FLUXNET sites are in North America, Europe and Asia, which were mainly in temperate and boreal ecosystems, thus, the more datasets from other experiment sites will be helpful to expand the multiple model. That's a good suggestion for further research.

Minor issues: - Please use the same symbols and terminology throughout the manuscript (e.g., potential evapotranspiration is later referred to as reference evapotranspiration).

Yes，we have modified it throughout the manuscript. (Line 138)

L 63: maximum stomatal conductance may be considered an ecosystem property, but actual stomatal conductance depends not only on vegetation types, but also on soil moisture, VPD, wind speed.

Yes，we have modified it. (Line 64)

L 149: LAI is not a biomass measure; it is linked to leaf biomass via the specific leaf area, but this parameter varies across ecosystems.

Yes，we have modified it. (Line 155)

L 159: months are very not meaningful when combining data from northern and southern hemispheres; rather, refer to summer and winter.

Yes，we have noted it before the submission, however, we forget change Fig.1 since we chose 3 EBF sites in North Hemisphere to modeling Kc. We have modified it in Fig.1 and Line 130.

L 194: as pointed out on P. 10, LAI and precipitation (and latitude) are not necessarily independent. A justification of the approach is thus necessary.

Yes，we have improved it. (Line 285-286)

Whiskers in Figures 2-3 mix different sources of variability – across locations and, for each location, across years. I wonder if it would be more meaningful to distinguish these two aspects.

Yes，Fig.2 and 3 are from the same data set, and especially Fig.3 is the seasonal Kc for the multiple modeling.