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Interactive comment

Interactive comment on "Examining the link between vegetation leaf area and land-atmosphere exchange of water, energy, and carbon fluxes using FLUXNET data" *by* Anne J. Hoek van Dijke et al.

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*A note upfront from the submitting person: This review was prepared by four master students in geography at the University of Zurich. The review was part of an exercise during a second semester master level seminar on "the biogeochemistry of plant-soil systems in a changing world", which is organized by prof. Dr. Michael Schmidt and myself. We would like to highlight that the depth of scientific knowledge and technical understanding of these reviewers represents that of master students. We enjoyed discussing the manuscript in the seminar, and hope that the comments will be helpful for

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Discussion paper

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the authors.*

The study by Hoek van Dijke et al. (2020) investigates whether the leaf area index (LAI) is a suitable predictor for modelling surface energy, water and carbon fluxes to improve climate models and extrapolations. The main question addressed is whether there is a link between LAI and surface fluxes for different land cover types along an aridity gradient. The authors hypothesize a strong link between LAI and surface fluxes in semi-arid and arid climates. For the statistical analysis, three datasets/networks were used. The aridity index (AI) derived from local flux towers and the LAI from MODIS data products. The flux towers cover five different land cover types (SAV, GRA, DBF, EBF & ENF) and provide five surface metrics (LE, H, EF, NEE & GPP). By using linear regression models, the relationship between each of these metrics and the site-year averages of LAI was evaluated. In addition, the effect of the AI on the relationship between the individual surface metrics and the LAI was evaluated. The main conclusion of the paper is that the link between LAI and surface fluxes depends on land cover type and aridity. Overall, the authors answered the research question and claimed that the LAI is a useful predictor for GPP and water and energy fluxes in SAV, GRA and EBF. Furthermore, there was a strong correlation between LAI and water and energy fluxes in arid regions and no or a weak correlation in regions with humid conditions.

The objective of using the LAI as a predictor for modelling and extrapolation of surface fluxes was thus achieved with reservations and can be used if the limits and uncertainties are taken into account. The research is particularly relevant in the context of climate change, its potential impact on vegetation properties and its influence on the carbon cycle. The text is reader-friendly, the structure is clear and the writing style of the paper is well chosen. We appreciate the broad data set used in the study to support the conclusions, as well as the detailed description of the data source, selection and processing. The authors make clear statements about the aim of the study, the research questions, the hypotheses and the possible results of the analysis. Furthermore, they continuously reflect uncertainties and limitations in the use of methods and

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indices. In principle, we think that the study fills some knowledge gaps, provides material for further research in this area and should, therefore, be published after some revisions. Below we describe our general comments to the manuscript.

First of all, we would like to focus on the structure and division of the chapters. In chapter 2, the data part (2.1) was very well explained, whereas the method part (2.2) only got one sentence of explanation. Our advice is to include table 2, which compares the two methods site-year and multi-year average, in chapter 2.2, and to explain there why the site-year method was chosen, to avoid confusions in chapter 3. The restructuring of the text will make it easier to understand which data were used to prove the hypothesis.

Secondly, the reliability of LAI is questioned. According to the authors, 62.5 % of the MODIS LAI is well estimated when compared to FLUXNET ground measurement data. However, in the remaining third of the data, MODIS LAI overestimated measured LAI on the ground. The question is whether it is reasonable to use MODIS LAI to study the link between vegetation and surface fluxes when LAI is an inaccurate index in determining vegetation characteristics. In this context, we could not find any statement or evaluation of a potential input error for the LAI in the regression model.

A third point is related to the methods of statistical analyses. Numerous past studies have used linear regression models to describe the relationship between LAI and surface fluxes. However, we partly question this approach, for example for GPP. At some point, there is a trade-off between primary productivity through photosynthesis and transpiration (closing of stomata to avoid dehydration in warmer or drier climates). Given that the stomata close at a certain level of moisture, the photosynthesis rate should slow down. Were the analyses also performed using non-linear models?

Finally, maybe a clearer focus and a reduction in factors would improve the comprehension. In general, we think the paper would be easier to understand when either water and energy fluxes or carbon fluxes were investigated and not all of the three.

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The paper mostly focuses on water and energy fluxes and only a few statements are made for the carbon fluxes. Focusing only on water and energy fluxes would reduce the complexity within the graphs and results.

Minor comments:

Line 103: How is vegetation disease defined? How is diseased vegetation identified (from the ground, remotely)? Why is diseased vegetation excluded? Maybe you could shortly explain your reasoning to justify the exclusion.

Line 283 ff: We struggle to relate the two main conclusions. In a) it is mentioned that LAI can model fluxes in SAV, GRA and EBF and b) that the link is strong in arid but weak in humid conditions. This raised the question whether this means by implication that the link is not good in humid SAV, GRA or EBF (but as shown in line 252 the link is strong for humid EBF). If the humid EBF is to be an exception, it would be beneficial to have a short sentence about this. Is it possible to assess which factor (land cover or aridity index) is the main driver of the link between LAI and water, energy and carbon fluxes? We suggest framing the conclusion more precisely to minimize such ambiguities.

Fig. 2-6: In most figures, the colors are difficult to differentiate, the data points are clustered and the regression lines are difficult to see. The readability of the figures would increase with higher resolution. We recommend using vector graphics (e.g. EPS format).

Fig. 3: According to our understanding, the colors for arid and humid grassland in the explanation were mixed up. Therefore, we think arid grassland should be in red, humid grassland in blue. Arid grassland is generally characterized by a low evaporative fraction (EF) and a low AI, while the opposite is true for humid grassland. Furthermore, it would also be helpful for the comprehension to have some further explanation for figure 3. We recommend to clearly explain for which reason this correlation was evaluated and how many of the arid and humid grassland were considered to draw the regression line (minimum 15 site-years line 165, 30 sites in caption, 20 data points for humid GRA

in figure).

Fig. 7: In the text (line 208) and the caption the abbreviation Rg is used for the short-wave radiation. In the y-axis you use Rn.

Table 1: We think the fact that multi-year averaged data is included in the table is confusing since in the caption it is written: Âń... for each site, mean yearly LAI & AI. . . are calculated for the included site-yearsÂż. In our opinion, it is more consistent (especially because yearly averaged data is used in the analysis) to include mean site-year averaged LAI and AI in the table and put it in the appendix. Otherwise, we advise adapting the caption for the table.

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