Reply to the students of the University of Zurich

We thank the students for the review of our manuscript and we thank Marijn van de Broek for uploading the review. Below we first reply to the two major comments raised. Afterwards you find our reply to the detailed comments. The review comments are given in blue, our response and proposed changes in the manuscript in black.

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.

Following a suggestion of referee #1, the results section will be extended with analysis on yearly data of a few sites to show the year-to-year variability in surface fluxes and LAI. It is a good suggestion to move table 2 to the methods, but given the extra analyses, we believe it is best to keep the table in the results section. In order to clarify the structure of the paper, we propose to add a few sentences to chapter 2.2. These sentences would be (line 164) 'To study the link between surface fluxes and LAI, we performed a linear regression between the surface fluxes and LAI. We calculated the correlation coefficient for 1) site-year data, 2) multi-year average data (site-to-site variability) and 3) yearly data for a few specific sites (year-to-year variability). Afterwards, to study ..'. In this way, chapter 2.2 outlines the structure of the results.

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.

MODIS LAI is indeed not the true LAI. None of the satellite derived LAI data products is perfect. MODIS LAI has a few advantages that made MODIS LAI the preferred data product. These advantages include the long record length, the good (and free) data availability, good spatial coverage, and high temporal revisit time. Furthermore, MODIS LAI is frequently used in land-atmosphere studies. The mentioned uncertainties in LAI (e.g. overestimation in some sites and saturation at high LAI) could introduce noise in the LAI data. We do however not expect this noise to change the direction of the regression models or increase the strength of the correlations. To the methodology (line 149) we well add: "Despite this overestimation, MODIS LAI was used, because it has a long record length, good (and free) data availability, good spatial coverage, and high temporal

resolution. The overestimation and saturation of the signal at high LAI could introduce noise in the LAI data. We do however not expect this noise to change the conclusions of our analysis."

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?

The analyses were also performed using non-linear regression models. For almost all surface fluxes, the data showed a linear distribution, and therefore, we performed linear regression. We agree that some relations are theoretically non-linear, e.g. for LE, that includes soil evaporation, transpiration and interception evaporation, is not expected to increase linearly with LAI at high LAI. In the range of surface fluxes and LAI included in our analyses, however, the relations show as linear.

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. 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

We thank the reviewers for this comment. We highly value easy-to-understand papers, and we agree that showing water and energy fluxes only would reduce the complexity. Several previous papers focussed on one of the two (water and energy, or carbon) and, to our knowledge, there is no similar research that combines these different parameters. We believe that it is beneficial to study the water, energy, and carbon fluxes together, as they are coupled. Also, the combined approach shows how the results differ for water and energy fluxes, as compared to carbon fluxes. We do admit that we could discuss the carbon fluxes in more detail, which we will do in a revised version of the manuscript.

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.

Two sites were removed because they were effected by a decade long beetle outbreak that resulted in high tree mortality and one heavily managed grassland site was removed. We will clarify this in the manuscript. This information was available from the online site information.

Line 283: 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.

Since land cover and aridity index are not entirely independent to each other, the two conclusions do go together. SAV is found in arid regions (and shows a strong correlation between LAI and land-atmosphere fluxes) and the different forest types are found in humid regions (and DBF and ENF show a weak or no correlation between LAI and land-atmosphere fluxes). GRA is found both arid and humid regions. For GRA, the relation is strong when all grassland land sites are studied together, but, as fig. 6 shows, the correlation is absent for H and EF when looking at the humid sites only. As mentioned in the conclusions, EBF forms an exception: the correlation is strong due to the probable role of interception evaporation, despite that most sites are found under humid conditions. We do hypothesise aridity to play a role in the strength of the correlation. With our analysis however, we cannot assess whether land cover or aridity is the main driver of the strength of the correlation.

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).

We will increase the resolution of the figures.

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).

Thank you for pointing out the mistake in colours. To clarify the figure and methodology, we will adjust paragraph 2.2 to 'To study if the link between LAI and fluxes changed with aridity, all site-years within one ecosystem type were ranked by aridity index. For each consecutive 30 site-years, we performed a linear regression between the fluxes and LAI. For some site-years, part of the data was missing that was needed to calculate the regression. Within each window of 30 site-years, the slope of the regression was calculated if at least 15 complete site-years were available.'

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

Thank you, we will change Rn in the y-axis to Rg.

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:

‘for each site, mean yearly LAI & AI are calculated for the included site-years’. 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.

The values for LAI & AI are the mean yearly LAI and AI for each site. We calculated the average for all years of data available in the dataset. We will change the caption to ‘For each site, mean yearly leaf area index (LAI) and aridity index (AI) are calculated for all years included in the dataset’.