Reply to referee #1

We thank referee #1 (R1, hereafter) for the review of our manuscript. Below we first reply to the two major comments raised. Afterwards you find our reply to the detailed comments.

The first major comment raised by R1 is that we discuss the vegetation control on water, energy, and carbon fluxes, while we only studied leaf area index (LAI) and LAI does not capture the whole spectrum of vegetation control. The water, energy, and carbon fluxes measured by flux towers are indeed influenced by vegetation through a combination of stomata, vegetation biophysical properties (shadowing, interception, energy distribution), and soil properties.

The objective of our manuscript is 'to get an insight about the intrinsic link between vegetation LAI and land-atmosphere exchange of water, energy, and carbon for different vegetation types across an aridity gradient'. Next to the discussion of the link between LAI and these fluxes, we aim to discuss the 'vegetation control' (one paragraph, line 258-267) and how LAI is implemented to model or extrapolate land-atmosphere fluxes (one paragraph, line 269-275). To clarify the text, we propose the following changes:

- line 19: change vegetation into leaf area index, line 21: change 'vegetation control on' into 'link between leaf area index and'.
- In the paragraph about vegetation control (line 258-267), we propose to change the first sentence as: 'Our statistical analysis cannot be used to study causality between LAI and surface fluxes, or to study vegetation control on the surface fluxes. The correlation between LAI and water fluxes is confounded by the effects of soil moisture, especially in arid and semi-arid ecosystems, where both canopy development and LE increase with water availability (Mallick et al., 2018). Despite LAI is related to vegetation properties, but not a direct measure of canopy conductance.'.
- In the conclusion, we propose to mention the difference between LAI and vegetation control (line 286).

The second major comment raised by R1 is that we study spatial (site-to-site) and temporal (interannual) variability simultaneously. As R1 states, seasonal variability in the fluxes and LAI are large. In most, if not all, ecosystems, vegetation proxies (LAI or NDVI) and fluxes are highly correlated over seasons: they show an increase in the wet, or warm season and a decrease in the dry or cold season. The objective of this manuscript is not to investigate this seasonal correlation, but rather we would like to study interannual variability.

In Fig. 5 of the manuscript we show that the spatial correlation for LAI and land-atmosphere fluxes is comparable to the spatiotemporal correlation. In the revised version of this manuscript, we propose to add a figure showing the temporal correlation of LAI and fluxes (Figure 1 - below). This is an illustration of the link between LAI and fluxes for ten different flux tower sites that have the largest number of available data. In summary, this figure shows that 1) Contrary to the spatial correlation, for we do not (often) find a temporal correlation between LAI and H, EF and GPP, 2) If a correlation is significant, it is of similar direction as the spatial correlation. For other flux towers, there is no significant correlation, and 3) Sites are clustered in the LAI-flux space; the site-to-site variability is usually larger than the year-to-year variability.

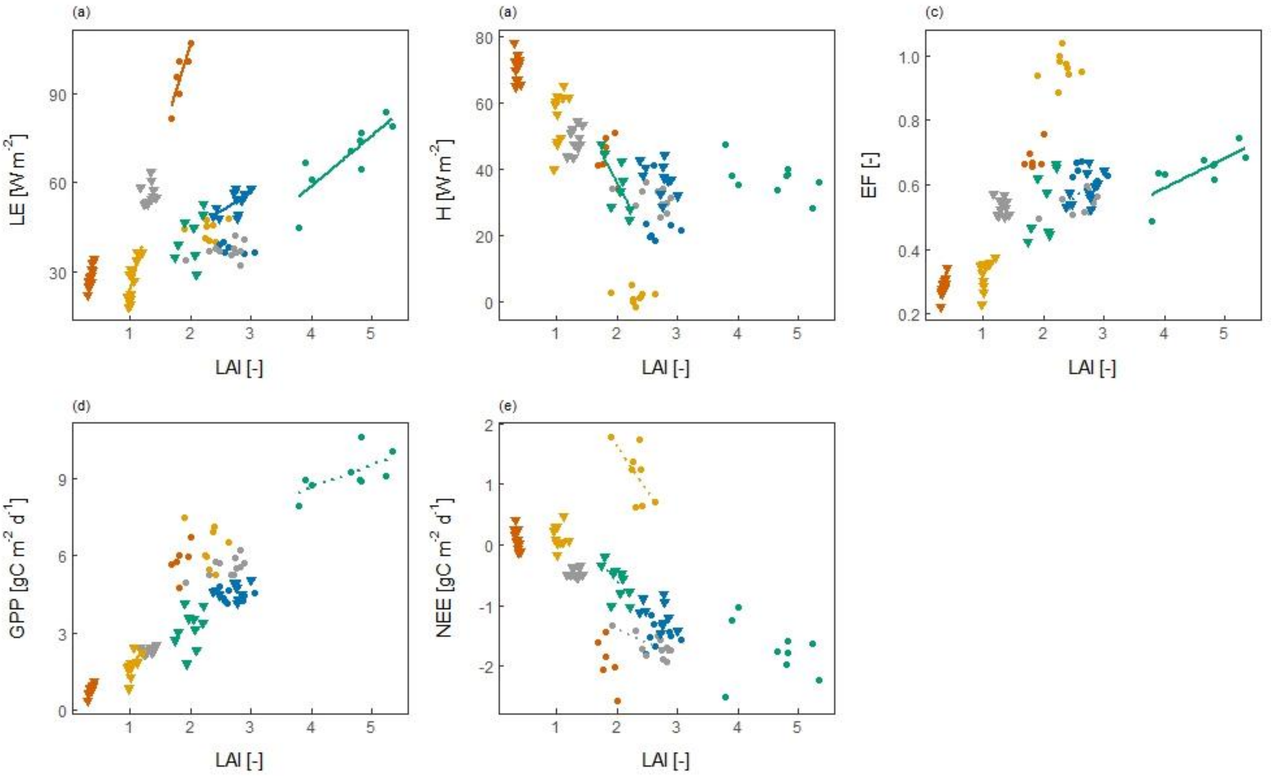

Figure 1: The relation between year-to-year variability in surface fluxes and leaf area index (LAI). Panels show (a) the latent heat flux (LE), (b) the sensible heat flux (H), (c) the evaporative fraction (EF), (d) gross primary productivity (GPP), and (e) net ecosystem exchange (NEE). For each land cover type, two flux towers were selected with the highest data availability, indicated by circles and triangles. A line indicates a significant correlation at $p < 0.05$ and a dashed line indicates a significant correlation at $p < 0.1$.

Below you find our response to the detailed comments, and how we suggest to implement them in a revised version of the manuscript (the review comments in blue, our response in black).

Line 16: what does ‘large-scale’ mean in this context?

We believe that large-scale is not the right term to use. Therefore we suggest to change the sentence into ‘We aim to study the link between vegetation and surface fluxes by combining MODIS leaf area index with flux tower measurements of water (latent heat), energy (sensible heat), and carbon (gross primary productivity and net ecosystem exchange).’ We will also remove ‘large-scale’ from the sentences in line 23, 24, 76, 267, 285, and 291.

Line 21: qualify that this is on annual average or interannual timescales

We suggest to change this to: “In contrast to water and energy fluxes, we found a strong correlation between leaf area index and gross primary productivity on both interannual and mean yearly scale. This correlation was independent of vegetation type and aridity index.”

Line 23: ‘insight into’

Thank you for the suggestion

Line 25: As noted above, the conclusion of the study needs as currently stated is more broadly worded than what the results and methods allow. Of course LAI is a necessary variable for modeling in order to scale photosynthesis and transpiration from leaf to canopy, so stating that it is not ‘useful’ is confusing. It may not be as helpful to consider LAI to be a ‘parameter’ either (line 64), in the sense of an adjustable factor or tuning knob. It is more like a variable that is either predicted or prescribed in order to model canopy-scale processes such as light interception. More specifically, what the authors seem to be saying is that LAI plays less of a role in explaining interannual variability of annually-averaged fluxes than other variables such as net radiation.

We will change ‘parameter’ in line 64 into ‘variable’. To constrain the conclusion to fit the method and results, we suggest to change the second part of the sentence in: ‘LAI is only of limited use in deciduous broadleaf forest and evergreen needleleaf forest to model spatial variability in water and energy fluxes’. We will also reword the conclusions of the manuscript.

Line 30: Is the phrase “on the other hand” necessary or appropriate? Maybe “additionally” is more appropriate, since there is not a strong contrast between this sentence and what came before?

We agree and we will change “on the other hand” into “additionally”.

Line 53: Was the cited reference a modelling study, or an analysis of model output? There are other references in which LAI was experimentally changed in models to show what impact it has on climate predictions, which could also be cited here; for example Boussetta et al. 2013, but there are probably others.
Boussetta, Souhail, et al. "Impact of a satellite-derived leaf area index monthly climatology in a global numerical weather prediction model." International journal of remote sensing 34.9-10 (2013): 3520-3542.
We will clarify that the results of Ferguson et al. (2012) were based on remote sensing data and models. Thank you for the suggested reference.

Line 56: "indicative of"
Thank you for the suggestion. We will change "indicative for" into "indicative of"

Line 68: The discussion of saturation of NDVI is appreciated and relevant to the interpretation of forest results. There is also potentially a slight nonlinear saturation of the effect of LAI on EF and LH that may explain the weaker correlation between the two on interannual timescales.
We could indeed expect to find a nonlinear saturation of the change in EF and LE with a change in LAI. At high LAI, an unit increase in LAI will correspond to a lower increase in energy availability (because of shadowing) and lower increase in LE as compared to a similar increase in LAI at low LAI. We do however not see this nonlinearity in the results.

Line 76: Again, I'm not sure what 'large-scale' means or what idea about scale the authors are trying to convey. What would be considered small scale? Do you mean canopy scale, as opposed to leaf scale? Flux measurements are not what I consider to be 'large-scale' from a meteorological point of view. Those measurements typically need to be scaled up to be interpreted at the scale of a meteorological model grid cell (100 km).
The sentence reads 'allows for a large-scale analysis of the link between vegetation characteristics and surface fluxes'. We agree that large-scale is not the right term to use and we suggest to rewrite the sentence into: 'allows for an analysis of the link between vegetation characteristics and surface fluxes'.

Line 108: "In some land cover types, the surface fluxes and LAI showed seasonal variation." This statement understates the importance of the seasonal cycle. More realistically, most land cover types exhibit some kind of seasonal variation. Some sites may have muted seasonal variations, but even tropical sites have a wet and dry season.
We will reword this sentence into: "For most sites, surface fluxes and LAI showed seasonal variation."

Lines 110-114: I appreciate this discussion of the nonlinearity and what it means to average over the seasonal cycle. However it is still unclear how this coarse-scale temporal averaging affects the results and interpretation. For example, for deciduous broadleaf forests, the winter months are irrelevant for inferring the stomatal control on latent heat flux, so why include those months in the analysis if the goal is to quantify the vegetation influence on fluxes? Are the conclusions (that these sites show little vegetation or stomatal control on annually-averaged heat fluxes, based on correlations) dependent on the fact that for more than half of the year there is no active vegetation present?

As R1 pointed out, the non-growing season might be non-relevant for finding the link between fluxes and vegetation. Using growing season data only, however, created difficulties. The different ecosystems have a different timing and number of growing seasons. Also, for some towers, the growing season did not overlap with the peak in LAI and fluxes. Because of the high variability between flux tower sites, using e.g. time series analyses to extract growing season data was not successful. We prefer to be consistent and use one measure of LAI and fluxes that can be applied to all flux towers, therefore we decided to use mean yearly values. We can elaborate more on this choice and the implications in the methodology section (line 109).

Figure 3 - I'm assuming that there is a mistake and 'arid grassland' should have red markers, and 'humid grassland' should have blue.

Thank you for pointing out the mistake in colours.

This figure could be described more clearly and with more information. What is meant by a 'moving window of aridity index'. What exactly do the markers represent? The caption mentions '30 site years...', and the paragraph (Line 165) mentions 'with a minimum of 15 site years for the lowest and highest aridity boundary), and figure itself shows about 20 data points for the humid and 23 for the arid, which is neither 15 nor 30. My best guess is that all the site years were pooled within ecosystem types (mixing different sites into the same pool), and then ranked by aridity index. Then, the correlation between EF and LAI was calculated for the top and bottom 30 most humid and arid site years. But then why are there only 20 or so datapoints?

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.'

Another question is whether the top and bottom years ranked by aridity are dominated by a small subset of sites (i.e., sites with intermediate aridity are not shown in Fig. 3), and what impact the site-to-site variation has on the results. For example, some ecosystems may be more productive or have higher water-use efficiency than others for various reasons (soil type and nutrients, age of stand, amount of photosynthetically active radiation, etc) even within a given ecosystem type (grassland, forest, etc). I suspect that for each site, there is indeed a relationship between LAI and EF, but the slope of that relationship is different for different sites even within the same vegetation type category. Some sites/species use their leaves more efficiently than others. If that were the case, then pooling all of the sites together could result in the weak relationships shown here. The 'all-year averages' shown in Fig. 6 indicate that most of the variation explored here is indeed due to variation across sites and not necessarily due to the variation in LAI alone.

The year-to-year and site-to-site variability is addressed above.

Line 171: It would help to know whether this result holds when calculating the correlation separately for each site. Either way, the discussion of these results should mention this issue.

This issue is addressed above.

Figure 7: Consider better notation such as r (Flux, P) to denote the correlation between the two, and likewise for r(Flux, Rn), and then in the caption specify 'The correlation coefficient (r) between surface fluxes and ...)'.

Thank you for the suggestion, we will change this.

Line 230: There is some good discussion here on the role of canopy interception/evaporation, which one would think would contribute to a stronger relationship between LAI and LH or EF in forests, but as the authors noted this is not the case for temperate and boreal forest in this study. Again, the discussion is good, but it remains unclear why this study finds such a weak relationship and whether this is related to site variability and the chosen interannual timescale. It is also worth noting that the LAI derived from NDVI is "green" leaf area index, which is not necessarily the leaf area that is intercepting rainfall. There may be 'brown' leaves that participate in rainfall interception but result in a smaller 'green' LAI derived from NDVI.

From our study we do not show why we find a weak link for temperate and boreal forest, but we do provide a few suggestions. We will adjust the discussion to clarify that we find this result for the site-to-site variability, as well as for the site-year analysis.

References

Ferguson, C. R., Wood, E. F., and Vinukollu, R. K.: A Global Intercomparison of Modeled and Observed Land–Atmosphere Coupling*, J. Hydrometeorol., 13, 749-784, https://doi.org/10.1175/jhm-d-11-0119.1, 2012.

Mallick, K., Toivonen, E., Trebs, I., Boegh, E., Cleverly, J., Eamus, D., Koivusalo, H., Drewry, D., Arndt, S. K., Griebel, A., Beringer, J., and Garcia, M.: Bridging Thermal Infrared Sensing and Physically-Based Evapotranspiration Modeling: From Theoretical Implementation to Validation Across an Aridity Gradient in Australian Ecosystems, Water Resour. Res., 54, 3409-3435, https://doi.org/10.1029/2017wr021357, 2018.