Interactive comment on “Surface response to rain events throughout the West African monsoon” by F. Lohou et al.

Response to Anonymous Referee #1

We thank both referees for their comments on our manuscript. Here is a detailed response to referee # 1.

The manuscript by Lahou et al. investigates the e-folding time scale of evaporation decay over the Sahel. I have been struggling to find the main novelties of the pa- per compared to preceding work on time scales of soil moisture (Teuling et al. 2006, Katul et al. 2007, Seneviratne’s work, Koster’s work and Dirmeyer’s work). I there- fore suggest major revisions. In many places the manuscript is not clear or goes into too much detail while missing the main points of the paper. The paper could be sub- stantially improved by systematically highlighting the major (new) points of the paper, in perspective with existing literature on the subject. Some sophisticated analyses of soil moisture time scale (in models) have been developed by Seneviratne, Koster and Dirmeyer. It would be important to discuss these paper, which try to decompose the soil moisture time scale into synoptic and soil dependence and how they relate to your work.

We agree that several studies have investigated the dynamics of soil moisture, the role of the different time-scale on soil moisture and water fluxes, based on data or models or both. In terms of scales, the primary focus of our paper is the evolution of evaporative fraction at the synoptic scale, meaning a few days. This scale is really important for convection triggering, boundary layer dynamics in West Africa during the monsoon. Some longer time-scales, like the seasonal time-scale, impact the root zone soil moisture and vegetation growth, which in turn, impact EF dynamics. We restrain the analysis of these longer scales to a simple 'bare ground / vegetation period distinction' for vegetation growth. This framework, although simple, proved to be convenient to make two important points: i) Plant life-form (perennial versus annual) is critical for EF dynamics in the vegetation season, and ii) LSM models differ surprisingly on bare soil evaporation dynamics in bare soil season.

The seasonal cycle of root-zone moisture is somehow present in the figures with soil moisture as the x axis. We have added, in the revised text, indications on the soil moisture regime (defined as wet, dry or transitional in some of the papers cited by reviewer 1). Moreover, we made it clearer that the primary is on synoptic scale, and why it is so.

We also emphasize the fact that our study relies on a flux stations network that samples a climate transect (different seasonal cycle at different latitudes, with a pronounced aridity gradient) and the dominant vegetation types. This is important to diagnose the processes or features that are the most relevant to West-Africa. The location of eddy covariance stations is often an issue, as can be seen from the global coverage of FLUXNET, with places like Africa, Russia or tropical America largely under-represented. The need of ground-based validation network in Africa is also pointed out by Seneviratne et al, 2010.

We hope that the major points and their relation to previous litterature will be better identified in the revised version.

Specific comments:

- page 18583 paragraph line 10-20: Beside rooting depth (which is really a model abstraction and works very differently in reality), there are lots of other factors that can explain the slower decrease of evapotranspiration in drier climate such as drought resistance (e.g. plant physiological traits, water use efficiencies...).

Teuling et al. 2006 have investigated the end-of-season drying with flux data and GSWP2 models. Their conclusions on the critical role of rooting depth are largely based on data analysis, not on models. They suggest to adapt effective rooting depth in models to mimic reality, saying that rooting depth on model may be a model abstraction as reviewer 1 said.

When it comes to the "slower decrease of evapotranspiration in drier climate" pointed by reviewer 1, this is not what we find. Indeed, we find an inverse trend, with the smallest e-folding times in the more arid sites (see tau1), at odds with Teuling's conclusions. We have an explanation for that, which we try to make more clear in the revised version. Teuling et al 2006 worked on a panel of flux sites scattered worlwide, from which they extracted important findings. We have a network of sites along a climate transect, which was designed to sample the main ecosystem types (namely the systems that occupy the largest surfaces). It turned out that the more arid sites are dominated by annual plants: either the Mali grassland, or the Niger crops. The more humid sites comprise larger fractions of perennial plants: shrubs in the fallow sites, trees in the forest site. Perennial plants with perennial root-systems are responsible for the slower decrease of EF in the more humid sites, not in the more arid. It is interesting to see how the two situations (Teuling et al. and our study) are consistent in terms of the importance of perennial root systems, but not in terms of climatic gradient, at least in West Africa.
In the revised manuscript, we attempt to make the point that the perennial/annual types of the plant is the most important factor for EF e-folding times.

Now, we agree that there are other traits (apart from the root system associated to the perennial versus annual life-form) that could contribute to different e-folding times along the transect. So far, in terms of stomatal conductance, there is evidence that arid sites in the transect have higher leaf conductance (results from the TROBITS project, Domingues et al. 2010, and unpublished data from Damesin and collaborators). Canopy conductances however may depart from this gradient (because of LAI and soil evaporation). We also have unpublished data showing that herbaceous plants in the Mali grassland tend not to close their stomata during dry spells or in response to VPD. That would fit in a conceptual model with shorter e-folding times in arid sites (rapid depletion of soil moisture du to minimal stomatal regulation). However, we believe we do not have strong-enough evidence, in terms of traits (other the annual/perennial life-form), to support or explain the results of our paper, so we keep to the annual/perennial character and root systems, for which we have strong evidence.

Domingues TF, Meir P, Feldpausch TR, Saiz G, Veenendaal EM, Schrodt F, Bird M, Djagbletey G, Hien F, Compaore H et al. 2010. Co-limitation of photosynthetic capacity by nitrogen and phosphorus in West Africa woodlands. Plant, Cell & Environment 33: 959–980.

- page 18587: give depths of soil moisture measurements

The depths of soil moisture measurements for each site were already indicated in table 1, introduced at the beginning of the paragraph. Table 1 will be again referred to when discussing soil moisture measurements in the text.

- page 18591: explain that EF is assumed constant and why we can assume that it is constant

We agree that EF diurnal variation must be discussed and we suggest the following modification (in bold letters) in the paragraph:

« The surface state is considered from the turbulent flux partitioning point of view, via the evaporative fraction. This limits the impact of temporal variability of net radiation on our calculations. H and LE are averaged to get the mean surface response over a 24-hour period of time. Only measurements between 0600 and 1800 UTC are used to get H and LE means from which EF is computed. **Several experimental studies show that EF can be considered, under stable radiation and low to moderate advection, as a constant during daytime hours, referred as « self-preservation » of EF (see Crago et Qualls, 2013 for a review).** D0 stands ….. »

- page 18592: 5-day long instead of 5 days long

This will be corrected in the new version of the manuscript. Thank you.

- above 4.1.2: you could mention that results could have been expected since EF is never close to 1.

We think that the cases for which the atmospheric demand limits the evapotranspiration are not automatically associated with evaporative fraction close to 1. That is why the result was not expected.

- page 18599 line 5: you should point out that this rooting depth is just a model abstraction to represent time scale of evapotranspiration and water use, but plants function very differently in reality. Maybe you should have cited some of the work of Rodriguez-Iturbe.

We hope that our work contributes to indentify critical parameters for LSM models in West-Africa. For instance, the importance of annual plants in grasslands, steppes, and cultivated area should be given high priority, as well a the density of perennial shrubs and trees. There is a high degree of uncertainty in these variables, for the present, but also for the past decades.

One last comment: maybe HESS would have been a better fit for this type of sub ject/study