

Interactive comment on “Evaporation from Savanna and Agriculture in Semi-Arid West Africa” by Natalie C. Ceperley et al.

Anonymous Referee #2

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Thank you for giving me the opportunity to review the paper "evaporation from Savanna and Agriculture in Semi-Arid West-Africa". The paper presents 17 months of energy fluxes data over the two dominant land covers found in the Sudano-Sahelian area of West-Africa, namely fallow/crop plots and woody savannah. Such data are highly valuable because they are very limited in the area, yet in this specific region the surface to atmosphere exchanges are complex to characterize while fundamentals for the understanding of both atmospheric processes and hydrological cycle. In the context of exceptionally high demographic rates and subsequent fast-occurring land use changes, there is a strong need to both document and monitor over long time surface fluxes for different land covers. This study is fully dedicated to that scope. The authors also seek to derive simpler estimates of evapotranspiration from evaporative fraction (EF) as a first step to obtain spatial evapotranspiration from remote-sensing products.

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Overall, I find the dataset presented of significant importance and the paper well written, and I do recommend publication in the journal. I do, however, think that there is currently too much ambition in the paper which somehow undermines its relevance. I see two main points currently developed in the paper: 1) the physical interpretation of fluxes data backed up by additional meteorological/hydrological/environment variables and 2) the development of a proxy-based method for regionalizing evapotranspiration based on EF. While the two aspects are given equal importance in the abstract, the first one takes a significant proportion of the paper, but could still deserve more development and discussion (see comments below). Some of the major shortcoming of this aspect is –to my point of view- the limited discussion with respect to other similar, studies undertaken in the area, and a slightly more comprehensive footprint analysis to discuss the high fluxes observed. On the other hand the second aspect is only limited to deriving an empirical relationship between EF and NDVI and soil moisture, and without providing an evapotranspiration map, for instance, which could be a nice achievement. The easiest way to deal with this point is probably to modify the abstract and develop some aspects of the physical basis of E fluxes, but I encourage the authors to develop more thoroughly the NDVI-based model (although so far, the EF derived time series seems unrealistically high to me).

Due to the large amount of minor comments that I have, I do recommend a major revision, but I think that the authors will have no trouble tackling all the little issues regarding to their dataset. I am not a specialist on energy budget analyses, and my comments on physical interpretation will therefore be limited, although I have the feeling that they could be strengthened.

I do appreciate the link with social aspects, although much care should be taken when interpreting local practices without dedicated studies. I am not an English native speaker, so I won't be commenting much on the English, and please excuse my poor English. And last but not least, this is a nice study, thank you.

Below are my comments:

Title: I see two minor issues with the title:

o Savanna is a land cover –and as such a source for ET-, but agriculture is a practice, and therefore not similarly related to ET. I would recommend using “Savanna and a cultivated area” or similar. I am not a native English speaker, and this might not be so important, hence feel free to take this comment into account or not.

o The area is really on the edge of what can be considered as semi-arid. I would rather relate it to its eco-hydro-climatic classification of the transition zone between the sahelian zone and the Sudanian zone: the sudano-sahelian area. (Sahel is roughly comprised between 100 and 700mm annual rainfall, Sudanian area between 700-1400mm, and Guinean >1400mm; and some authors consider a sahelo-sudanian or sudano-sahelian transition band, which I think this region could fall into. Sudanian works too.

Abstract:

o L 20: I am not sure that you actually showed evidences that the fact that fluxes above the savanna-forest where higher was due to the number of rocks and trees and tree productivity. A comprehensive footprint analysis would, at least, be necessary.

o L25: I think a paragraph is missing in the paper, as Figure 12 is not explained, and it is never written in the main text that NDVI only is sufficient to predict EF. This should be also further discussed.

p2.L2: This is not straightforward: it should be sustained by a proper reference or moderated by adding “for example”. For instance, there could be among-species differences in leaf renewal timing for some trees, not necessarily related to moisture availability.

P2.L18-25: I would move these equations to section 2.3 (as is done with EF- or move EF equation to the introduction, although this relates more to “measurements and calculations” than to an introduction).

P2.L26-32: consider removing some references. 2-3 references by statement are

enough.

P2.L32: Let me suggest a few references that should be carefully studied and probably discussed at the end of the article, because they are located either in similar eco-climatic context, or bounding (either in the south or in the north) the study area. (Brümmer et al., 2008; Guyot et al., 2009, 2012, Lohou et al., 2010, 2014, Mamadou et al., 2014, 2016; Velluet et al., 2014)

P3.L6: It is not the evaporative fraction which is based on the concept of self-preservation, but “the method that estimates daily evapotranspiration from evaporative fraction”

P3.L15: This has been studied in West-Africa and for a broad range of eco-climatic contexts by (Lohou et al., 2014)

P3.L19: is “raised” plateau an appropriate term? I have never heard it.

P3.L20: This is not true. I have not checked all the literature, but at least (Guyot et al., 2012; Lohou et al., 2014; Mamadou et al., 2016; Velluet et al., 2014) have studied energy fluxes in the area for different land covers and several seasonal cycles.

P3. L24-L27: I would move that to the conclusion.

P4. L 16-17: I am not sure this is relevant here.

P4. L18: It is not the sudanian area which is defined by alternating wet and dry season, but rather the monsoon climate. My suggestion would be (if you choose to keep sudanian instead of sahel-sudanian): “The watershed falls within the sudanian zone of the west-African monsoon climate system, with alternating dry and wet seasons with the rainfall falling. . . .”

P5.L7-8: This is an important remark: why not using ground T° being recorded with the Decagon moisture probes for calculating G? Only two depths are enough for a first calculation using the Harmonic method (Guyot et al., 2009). This would allow to be

less elusive about the residuals of the energy budget, and to bring more evidence to conclude on higher/lower G under fields/Savanna.

P6.L6: I really think that individual times series should be shown, because according to Figure 5, they can be much different. At least one for the forest-Savanna cover and another for the field cover, and for the two depths, on a same figure containing H, LE and RN for the two land covers and for the same period. Maybe NDVI too?

P6-7: section 2.8; I do agree with Reviewer #1 who proposes to remove this section. It could bring some interesting results, but would need much more developments, which are not the purpose of that paper. This would allow to gain some room for further developments in the text.

P8.L10. To me, the NDVI for the forest in the dry season is not as much greener as you would expect from a forest dominated by *Vitellaria paradoxa*, which only need about a month for leaf renewal. The NDVI pixel must include some significant herbaceous cover. If possible, could you estimate the vegetation classes distribution within the pixel?

P8.L17. These are not average diurnal cycles, but single days samples. For me, all the results and conclusions drawn from this analyze (otherwise very interesting!) are weak due to the undersampling of diurnal cycles. For instance, we could not state from these single days, that “Net radiation is slightly higher during the wet season (P8.L21)”, (although this is probably true). If there is enough data to produce composite diurnal cycles over larger periods, please do. It is very hard to understand why there is such a significant LE flux in april over the field, which should be composed of bare soil according to the text and pictures. I can only suspect that there has been a rain in the previous day(s), - and there has been rain events in april according to Fig.4, and that the single day sample is too particular to be analyzed as a representative day. There may also be some other processes acting, such as lateral subsurface water transfer, and that should be discussed, but I doubt so.

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Another solution is to provide time series of RN, LE and H (and SM, rainfall, NDVI), as proposed on the comment P6.L6, on a larger plot than Figure 8, and not separated by wind sectors.

These results should be discussed in the light of previous results for diurnal cycles obtained in the area (Guyot et al., 2012; Mamadou et al., 2014, 2016), for instance.

P8.L25: there is also, for both sites, this interesting feature in the late afternoon that $LE > RN$ and even slightly positive during the night, as noted in (Mamadou et al., 2014, 2016), for instance.

P8.L31: It was lower over the field (if I am not mistaken).

P8.31: again, the expression “for all months” does not hold when only two days are compared.

P9.L2: this is an interesting statement, but it needs to be further supported. For instance, (Mamadou et al., 2016), for a forested site in similar conditions, noted an early LE peak in the dry season, but also that Soil moisture changed very little from the morning to the afternoon (< 0.1 mm). They concluded that it was probably a temperature limitation of stomatal opening.

P9.L2-7: this is not supported by a figure. Again, I think that time series of the fluxes should be shown. Also, I think the discussion on limitations should consider here conductances, which can be affected by several processes, such as moisture limitations, but also shading, or stomatal resistances.

P9.L5. What is the “absolute maximum”?

P9.L11. I am not sure about this conclusion. I think that dynamic aspects should be taken into account, as fields and savanna will not necessarily have equal amounts of RN at the same time. I am not entirely sure on this, and I think that calculating G from temperature probes could help on that.

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P9.L15-19. I think this is similar to what is found in (Guyot et al., 2009)

P9.L20-28: This relates to the footprint concept. To my point of view, the footprint should be explicitly calculated for specific periods. But I would understand that it could significantly impact the paper in its current form. At least, wind sectors of Figure 8 should be more discussed; For instance, I do not clearly understand what is the difference between all the time series of, say, the Field panel, how have they been calculated? Also, I may be wrong, but I guess that N-W winds are the Harmattan winds, bringing hot, dry, and dusty air from the Sahara, and S-E winds would be the monsoon inflows, bringing moist air? If so, and as this has significant impact in the resulting energy budget, it could deserve some further insights (see e.g. (Guyot et al., 2012)).

P9.L25-27. I agree with Reviewer #1 that Figure 7 does not bring much, thank you for agreeing to move this figure to the appendix. Also, I am not sure that such correlation is an evidence for a causal effect.

P10.L2. Burning practice should be defined in the site description.

P10.L11-15: thank you for the correlation coefficient added on this figure.

P10.L25: My guess is that there are serious temporal limitations in this approach, and although the correlation coef are not too bad, the approach could probably be improved by considering different time windows. For instance, in the field, there is bare soil in the dry season, and for a little while, only soil evaporation may occur, and there is probably no need to take NDVI into account. On the opposite, once the herbaceous layer has grown, the system may not be water limited anymore, and soil moisture is not needed anymore. During the growing phase only, the equation could probably produce better results.

Also, what soil moisture time series have you used? If this is the catchment averaged one, then the higher regression slope on soil moisture for the field further indicate a

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higher moisture-controlled system. If the ultimate goal is to compare it with remote sensing-derived soil moisture, this could easily be added here.

P11.L4-10: some statements need to be made clearer (but maybe this is due to my poor english):

- o 'sensible heat flux showed the greatest diff. in November & March': agree for November, although October seems even more different, but the ratio in March is close to 1. Unless the two mentioned surfaces are the two field surfaces (2009: millet, 2010 fallow)? But then it should be June and September.

- o Again, for LE flux: the strong difference between forest and field are in March. I guess you point to the millet and fallow land use? Could you further discuss on why the fallow evapotranspire less?

- o L7-9: I think again Figure 8 is not clear enough, why not having a plot (as suggested above) with the fluxes of forest + field on a similar panel? There could be a panel of Rn, of LE, H, Soil moisture(+precip), and, say, NDVI?

- o L9: The strongest difference could also be expected in the dry season, no? (see e.g. (Mamadou et al., 2016)) Or please expand on why this is expected.

- o On Rn: (Guyot et al., 2009) described a higher Rn on the dry season over woody cover, because surface temperature was lower, and LWnet higher. This could be discussed (if judged relevant), because there is an opposite behavior here. Although according to Fig.7 LWnet is higher in the forest in the dry season, and there are two tails in the LW-net box plot of Figure 6.

P11.L15-16: I am not sure why rock presence should be mentioned here. Plus, they are not the only producers of H fluxes. Maybe an extra sentence could be added? And to clearly show that they are somehow responsible for a good part of H flux, a footprint analysis should be undertaken.

Also, according to Figure 1, there is a strong topographic difference close to the

forested area. I am not a specialist, but could that affect fluxes in any way?

P11.L18: this is not clear to me: are we talking about trees getting water from open waters in the channel? If not, it should be simply stated that upper trees have 'probably' access to shallow groundwater at the origin of the springs.

P11.L20: what is permanent?

P11.L20-21: I guess reference to the land cover is missing: this sentence relates to the field, right?

P11.L26. According to the picture in Figure 5, the field location is not particularly covered with more vegetation. The LE fluxes there are really high, and I have no explanation for this. On a Large Aperture scintillometer beam pathway, including very mixed cover, and including also a gallery forest preserved for similar reasons (Guyot et al., 2012) had much lower LE fluxes for instance. Same observation for (Brümmer et al., 2008) in South West Burkina Faso, at a similar latitude, or (Mamadou et al., 2016) in a fully forested parkland in North Benin. This is very surprising, and I have no explanation.

I am also curious, I have looked at the satellites images in google map, and the field location seems to appear in a middle of a much larger forest. What is at the origin of this specific deforestation? (this is not relevant in the review, only personal interest).

P12.L5-8, this should be rewritten according to the references given in the comment P2.L32. There is a site in Burkina Faso in similar climatic context, and sites in Benin are not that far south, and as for the cultivated ones, have probably a rather similar ecological context.

P12.L11-18: I have read that references to figure 12 have been added, as well as more discussion. Currently there is a strong issue that really needs to be addressed: The EF clearly shows that fluxes are very high, and this should be commented. EF could be compared with the study of (Lohou et al., 2014), which spans different eco-hydrological

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contexts through the N-S West-African gradient. In your Figure 12, EF does not seem to drop below 0.5, which is exceptionally high. If I roughly take a yearly average value of 0.6, and a yearly average daily value for ETo of say 5mm (Wang et al., 2007), which is probably a lower bound for ETo in this area, this produces about 1100mm or evapotranspiration yearly, which is much higher than precipitation. Footprint analysis could be undertaken to identify the reason for such high evapotranspiration. This could lead to very promising result.

P12.L23: cultural taboos, or health-related issues (malaria)?

P13.L8: Add “in the field”, to make it clearer

Figures:

In general, figure captions are too long and should be limited to description and not interpretation, but Figures are of good quality.

Figure 1: If possible, I would prefer the elevation shown as contour lines to better see the satellite image below. For instance, it is impossible to locate the village. And also, it is not easy to understand what is the dark line S-E / N-W just north of the study area: is it a plateau edge or a riparian area? I would recommend to put contour lines of the whole map, and draw the catchment contours with a proper shape.

Remove “land locked country” and 100m of elevation change. . .and the plateau”. If this is important, it should be put in the main text.

Figure 2: Figure caption: you could remove the last sentence, which is interpretation

Figure 4: soil moisture is exceptionally high in 2011 (known to be a dry year in the area, so I doubt there is any issue with the rain record), could you comment on that?

Figure 5: thank you for putting in the same figure both photographs and diurnal cycles, this makes a nice figure.

Figure 6: Figure caption: you could remove interpretation sentences. I also think there

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is some confusion in the middle-bottom two description.

Below are some technical corrections: P2.L1: change sudanian to sahelo-sudanian
P2.L16: add “in” before Eq.(1) P3.L7: [. . .]the diurnal cycle “of” each [. . .] P4. L3: [. . .]
were made in “a” small [. . .] P4. L21 “in the” Guinean zone P6.L9. remove “infrared”
P7.L4. where “smoothed” P8.L5. remove first occurrence of “air”. P8.L21. for the both
sites? P8.L25. Heat instead of heath P9.L12: remove “below” P9.L21: contributed “to”
higher fluxes? P10.L24: be fitted? P11.L16: at the end of the “wet” season? P12.L27:
has protected “it”?

Thank you again, this is a nice data set, and nice work.

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