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Discussions

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

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Dear Anonymous Referee 2, Thank you very much for your comments regarding our manuscript, 'Evaporation from Savanna and Agriculture in Semi-Arid West Africa'. I think your corrections will greatly improve the quality of the manuscript. I can incorporate most of them without any trouble.

You recommend developing a foot print analysis and an evaporation map, both of which are good ideas. However I feel they are best suited to future publications. As Hans Peter Schmid (2002) describes, foot print analysis over vegetation and over uneven topography is difficult and hard to validate. This could be addressed in comparison with the calculation of evaporation from our distributed meteorological stations that we hope to publish soon. Similarly, an evaporation map of the catchment will fit well in this

calculation as well. We could use these two results from this work and measurements in combination with our spatial measurements to determine how evaporation varies spatial and the footprint of our stations. This future publication would be dedicated to the spatial variation in evaporation.

On the other hand, based on your recommendations, which were quite helpful, I have added some more development and discussion, in particular regarding additional work that has come out from the region.

> 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).

As you recommended, I have made it clear that our fitting of evaporative fraction to NDVI and soil moisture is more of an exploration of the physical basis of evaporation and not a full scale model, ready for large scale implementation.

Your comments regarding the title are very helpful and I will consider changing it to "Evaporation from Cultivated and Semi-Wild Sudanian Savanna in West Africa" because based on the floristic composition of the site, we are solidly in the biogeographic / phytosociological zone, and not the sahelian or Sudano-sahelian. Upon request, in future publications, or in supplementary materials, I could provide general botanical inventories that we completed early in our time on the site that support this classification. Semi - arid refers to the highly seasonal precipitation pattern.

> Abstract: L 20: I am not sure that you actually showed evidences that the fact that fluxes above the savanna-forest were higher was due to the number of rocks and trees and tree productivity.

We did measure higher fluxes and explain it with the tree productivity.

> 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.

Thank you for bringing this to my attention. I have reconfigured some of the figures as per the recommendations of the first reviewer and I think that the connections between the figures and the text will be more apparent.

> 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.

You are right. This sentence is meant to be rather general pointing out out that there are some evergreen species on our site but most are deciduous. I changed the wording to make it clear that this is a general statement.

> 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).

This is a good suggestion. I moved the equations to the Flux Calculation

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

While I think this is usually true, in this case, I want to demonstrate that it is an ubiquitous problem.

> 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. (Brummer et al., 2008; Guyot et al., 2009, 2012, Lohou et al., 2010, 2014, Mamadou et al., 2014, 2016; Velluet et al., 2014)

Thank you for these references. I will add some discussion at the end of the article.

> 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?

Thank you, this is clarified.

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

Thank you for the additional references. I will incorporate them appropriately.

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

you are correct, the "raised" is redundant. I will remove this.

> 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.

Again, thank you for the additional references. I will incorporate them appropriately.

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

Thank you for this recommendation.

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

Since I do discuss social perception, I think it is relevant. The ethnic identity is the main way that the village identifies itself and is a primary determinant of land use.

> 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 sahelo-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. . ..'

This is a helpful precision, Thank you.

> 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.

This is a good suggestion. I will add this in current or future publications.

> 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?

Thank you for the suggestion. I will reorganize some of the figures to have a figure that just has time series.

> 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.

Thank you for this remark, it is helpful to make a final decision.

> 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?

This is a good suggestion, it is clear that all of the pixels are very mixed. I will try to include a vegetation class estimation in this publication or in the planned publication about the spatial variation of evaporation. There is no where in the catchment that the forest is dominated by V. paradoxa. V. paradoxa is an agroforestry tree most often found in fields outside of this study area.

> 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. 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.

Thank you for the suggestions for data presentation. It is clear that picking representative days can never capture the amount of variation visible. In a previous iteration of the paper, I did have a composite day, but we removed it because it concealed too much information. The scatter plot comparison of the components of the energy balance shows the information the comparison that would be visible on a composite plot, though not the diurnal variation. Figure 9 breaks up these comparisons by individual month. I will reconsider the choice of graphics once I make a time series plot as you suggest.

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.

Thank you for identifying this comparison, I will discuss it more in my improved comparison to other sites.

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.

According to figure 9, which is a composite comparison of all the months individually, shows that the ratio of Rn-ag to Rn-forest in April is less than 1, which means that it is lower over the field. Thank you for catching this mistake.

P9.L2: this is an interesting statement, but it needs to be further supported. For in- stance, (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 (<0.1mm). They concluded that it was probably a temperature limitation of stomatal opening.

Thank you for pointing me to this work.

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.

Thank you for the suggestion, I will include this in my revision and consider how to best support it with a figure.

> P9.L5. What is the 'absolute maximum'

Thank you for pointing me to this imprecision and confusion. I will rewrite it.

> 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.

This is also a good suggestion, I will qualify it and think about how to support it better (or refute it).

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

Thank you for identifying this comparison. I'll explore it more in depth.

> P9L20-28: 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)).

The wind sector analysis was a strategy to interpret the data based on the wind direction without a foot print analysis. Thank you for pointing out the aspects that are not clear, I will explain it better and further put it in context based on current literature.

> 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.

I will. Thank you for the suggestion.

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

These are very good suggestions. I would like to refine the model more, however perhaps it is beyond the scope of this paper? I will try to explain my soil moisture time series better in my revision. I did use a catchment averaged soil moisture, which does mean there is some bias, since there were more sensors and better working sensors, in the cultivated field.

> 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 evapotranspirate less?

Thank you for these comments, this is helpful to know where I need to go into more depth in my discussion. I'll improve these passages. It should be understandable even to a non-english speaker!

> 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?

Thanks for the suggestion. I'll see how I can improve this figure and the time series in general. Perhaps your other suggested figures will also improve this point.

> 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.

I would think this would be the highest difference because one site is still "moist" and the other is dry at this time. I'll see if Mamadou's findings help me explain this in a more relavant and convincing way.

> 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.

I'll discuss it in contrast to Guyot's findings. This is very helpful.

> 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?

Yes you are right. This is why I performed an extra coordinate transform on the data, which goes beyond the normal procedure of data correction. I'll try to pull up some more examples of how rock presence changes the energy balance.

> 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.

Yes, that is a smart way to word it.

> P11.L20: what is permanent?

The spring, I'll clarify.

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

yes you are right, I'll clarify

> 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 (Brummer 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'll look more closely at each of their cases and try to offer a more convincing 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).

The area all around this site is preserved as hunting reserves and Arly National Park. When people were forced to stop living in the areas as the became reserved, they were

granted permission to live and farm only on this small "island" inside of the conserved area. I am sorry I didn't make this history more clear. As a side project during this research, I collected some oral histories of this transition. I hope to publish something from these interviews as well, although it will probably be in a different journal.

> 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.

Thank you again for pointing me to that work.

> 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 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 evap- otranspiration 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.

Yes, thank you for this precision. Perhaps I will add a table to directly compare our results to others to try to pull out reasons why ours are so different. I will think about adding a footprint analysis in the future.

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

I am basing this on the best ethnography that I found of gourmantche agricultural practices. I did not study the full motivation behind historical taboos.

> P13.L8: Add 'in the field', to make it clearer

Thank you for this suggestion

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

Thank you, I will try to shorten them and put more in the text.

> 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.

Okay thank you for this, it is also in the main text, so I'll remove it here. I'll try to improve the map.

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

Okay, I will put it in the text.

> 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?

This is a good point. I'll try to find an explanation. But if I can't, it is worth pointing out this inconsistency.

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

You are welcome.

> Figure 6: Figure caption: you could remove interpretation sentences. I also think there is some confusion in the middle-bottom two description.

Okay, I'll clarify and move interpretation to the text.

> 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, these are helpful.

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