Review HESS

Assessment of actual evapotranspiration over a semi-arid heterogeneous land surface by means of coupled low resolution remote sensing data with energy balance model: comparison to extra Large Aperture Scintillometer measurements

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Paper Summary

This paper presents a multiscale study of evapotranspiration (ET) in semi-arid area of central Tunisia where agriculture production is limited by water availability. The analysis involves ecosystem fluxes obtained by means of eddy-covariance (EC) instrument and landscape fluxes through XLAS and satellite remote sensing. These observations have been combined to produce large-scale area-average estimates of ET and sensible heat fluxes through the use of a vegetation model driven by remote sensing input SPARSE. Results in terms of divergences in H and LE estimates between model and instrumentation are above 50 W/m2.

Considering the uncertainties and limitations bounding the present ET estimates on heterogeneous terrain; water stress was estimated within reasonable ranges.

Review Summary

This paper presents interesting results combining area-average large-scale observations of surface fluxes and modeling tools. Results in terms of the uncertainties on LE and H are within what has been observed on intensive field experiments in complex or perhaps, more precisely, highly complex environments as compared to the present one.

This review finds this paper border line between rejection and major reviews.

To substantiate this evaluation, this review finds an important number of instances in which the paper is not clear or need to be corrected or needs to provide better justification as well as improve clarity. Since the number of corrections found may take an important amount of time for the authors to resubmit that's why I'm giving the editor the options of rejection (with resubmission) or major correction. The results are definitively important but they need a much improvement.

Depending upon editor's decision I would like to see further:

1) Figures with better accuracy in their representation. For example, some of them seems to have been the result of quick spreadsheet plots but without including accurate axis ticks, grids, labels, etc.

2) Same as for the description of the figure captions and legends. The reader needs to understand a given figure by analyzing the figure and reading the information on the figure caption and legends.

3) A better explanation of the SPARSE methodology is needed, steps and the set of equations in the ET and H estimates. What the assumptions are and what is the physical framework? All of that is missing and therefore theoretically this paper is very weak. For example, from where the authors got a threshold value of 30 W/m2 to start the iteration? How convergence is achieved is a mystery here and how many iterations and how signal-to-noise ratio of RS data plays a role in that convergence? Which equation provides convergence we don't know.

4) I would like the authors to provide adequate justification to the use of formulas to deduce H based on LAS or XLAS. Particularly since the indicated formulas are valid only under the similarity hypothesis of Monin-Obukhov which implies homogenous surface and stationary

flows. No justification was provided as for how these conditions were tested to render valid the resulting HLAS flux.
5) when the authors discuss about uncertainties it is not clear what kind of uncertainties we are talking about and how have those been calculated? Moreover, uncertainties in heterogeneous terrain based on pure observations XLAS have not been computed. A reference is provided so that the authors can check on that.
6) Not clear where the EC flux comes into play. Also footprint functions for the scintillometers need to be accounted for. Reference on this element is provided below.
7) I would like the authors to provide an in-depth description of physical processes explaining the results in the final figures. Description of what is being presented in the figures is fine but we need more science here.

As an aside note the use of XLAS is not unique in this problem. A LAS can do 5 km max. optical beam path and resolve the same situation. What is critical with using XLAS is beyond 5 km optical path.

Bai, et al., 2015. "Characterizing the Footprint of Eddy Covariance System and Large Aperture Scintillometer Measurements to Validate Satellite-Based Surface Fluxes. *Geoscience and Remote Sensing Letters, IEEE,* 12(5), 943-947, 2015. doi: 10.1109/LGRS.2014.2368580.

Comments in details
Line 45 –off : please put references in chronologic order. This is the proper way to recognize previous work; unless specific discussions are provided which in those cases the trail of references needs to be broken down. This note is valid through the entire paper.

Line 50: About the claims about water scarcity related to climate change. -or better say climate variability: I wonder how compelling are these claims? – Can the authors substantiate in more details about this problem in this area? This is an important claim and need to be fully addressed by the authors to build context to this research and the methodologies being used.

Line 53: the use of "greatest" here tries to indicate what? "the larger" or "the most important"? This needs to be clearly understood without ambiguity and therefore we need to bring more specificity.

Line 56: I'll add complexity in the hydro-meteorological processes. As we move from ecosystem scale to landscape scales surface heterogeneity but also dynamic of the flow, cloudiness, precipitation come into play more aggressively. This also bring more context to the need of this study.

Line 61: I would disagree that "RS techniques becomes essential". Basically it has been demonstrated that plot (or ecosystem) exchanges within same complex canopies do verify consistent differences in sensible heat fluxes (the simplest and ubiquitous flux on earth) over distances that are much smaller than the RS footprint in particular MODIS. See Starkenburg et al., (2015).

Starkenburg et al. 2015: "Temperature regimes and turbulent heat fluxes across a heterogeneous canopy in an Alaskan boreal forest". *J. Geophys. Res. Atmos*., 120: 1348–1360. doi: 10.1002/2014JD022338

Now, I do agree that RS brings a mean to deduce, within certain ranges, an approximation of fluxes. What about mesoscale models? Or perhaps you wanted to indicated physical models using RS data as input? In any case, I think you should open this perspective here since there are other disciplines other than Remote Sensing Researchers that can also provide the same product.

Line 63: vegetation physical properties or characteristics?

Line 65: Authors use "plot" as one of the scales in which I assume results would be obtained. However, at no point plot-scale was defined. Please whenever plot is used for the first time in the Introduction section for example please clarify that. (excluding the abstract).

Line 87: please rephrase the text between parenthesis.

Line 93: Spell out FAO. If it is not being used anymore in the text, then no need to define an acronym.

Line 98-99: get rid of parenthesis here. What is inside is part of the phrase.

Line 102: FAO-56 put a reference here. Or make a short phrase explanation.

Line 103: what is "dry down"? please make sure you check consistency in all phrases.

Line 114: What's the meaning of adding quotes here? If single-source means single source, then no need for quotes. Quotes are used when you use a word or combination of words but you would like to indicate a different meaning.

Line 116: same as 114.

Line 117: comma missing before etc.

Line 128: add "they provide area-averaged sensible heat flux"

Line 130-131: incomplete phrase. And, can you elaborate a little bit more here?

Line 132: delete space before comma.

Line 133: representative of the pixel? It may be the case that for a particular MODIS data your scintillometer data intersects several pixels. Then we are talking about several pixels.

Line 140: **large-scale area-average** this is the proper measurement that one obtains from a scintillometer.

Lines 140-143: Here I need help. Are you indicating that to get ET large-scale area-average you use XLAS? But you need to assume a closure fraction or assume is 100% Energy Balance closure. As we increase surface heterogeneity and the atmospheric flow acquires an increased space-time variability then it is difficult to assume 100% energy balance closure. How you do then? Please explain how you treat and eventually circumvent this problem.
See for example Foken et al., (2006; 2010) and Foken (2008).
Foken, T., F. Wimmer, M. Mauder, C. Thomas, and C. Liebethal, 2006. Some aspects of the energy balance closure problem. *Atmos. Chem. Phys.,* 6, 4395–4402.
Foken, T., 2008: "The energy balance closure problem: An overview", *Ecol. Appl*., 18(6), 1351–1367.
Foken, T., M. Mauder, C. Liebethal, F. Wimmer, F. Beyrich, J.-P. Leps, S. Raasch, H. A. R. DeBruin, W. M. L. Meijninger, and J. Bange, 2010: "Energy balance closure for the LITFASS-2003 experiment", *Theor. Appl. Climatol.*, *101*(1-2), 149-160, doi: 10.1007/s00704-009-0216-8.

Line 146: what is the "layer" approach? Can you be more explicit and detailed? If layer is the name of the approach, then no need to use quotes.

Line 147: when authors normally explain the use of electrical resistance as equivalent models really are not paying attention to the details. So then now you need to explain how you transform an electrical element such as a Resistor, which is a concentrated parameter into a distributed vegetation or soil representation. What are the assumption? Hypothesis? Regions where this approximation is valid and where it fails, etc. I'll give you a hint R=V/I where V(electrical voltage: what is imposed the potential) and I(electrical current, what flows between the boundaries). Then when you say you use Rsoil and Rveg. What are the analogs of V and I here? What R actually means? And how you walk out from the Ohm's Law for concentrated electrical parameters and transition to our problem where these parameters are distributed?
This comes from Norman and Kustas TSEB- way before SPARSE.
For example, here it is important to remark that vegetation information has to be at much higher resolution than the radiometric information to account for vegetation/forest variations for example the existence of clear areas within the forest or cultivars. How the authors account for that needs better explanations. And, what assumptions underlain these approximations?

Line 150: I wanted to be clear here that XLAS ONLY can deduce sensible heat not LE. Please make sure this thread is conveyed all the way through your work.

Line 158: put "(" to indicate the reference the cultivars are within the phrase.

Line 173: what "double device" means for you. Please be specific.

Figure 2: it is not clear where the XLAS emitter and receiver are specifically located. Put a dot or a symbol to indicate that. Photos actually say nothing here. Now I see that the CSAT is close to the XLAS receiver. I would caution the authors here that any interpretation between XLAS fluxes and EC-CSAT fluxes would not be representative since the EC system is closer to the XLAS receiver and/or transmitter for that matter is the same.
More importantly what is not clear here is what are the green contours indicating the footprint? And if these are EC footprint more likely are wrong.

Please specify what SPOT5 bands 1,2,3 are in terms of wavelengths and they are used in this work.

Line 196: I would write Extra Large Aperture Scintillometer (XLAS)

Line 198: Phrase: “Scintillometer is based on the scintillation method” what is this?

Line 198-200: What is the cause and what is the effect? This phrase is wrong please think about a little bit.

Line 205: replace “bean” by “beam”

Line 204: The reference that links scintillations and Cn2 is given by Tatarskii. We need to give the proper reference here. The fact that those references have been using it doesn’t mean they were the ones given the foundation for this relationship. We need to make sure we give proper value to the actual references.

Line 206: symmetrical to what? What is that symmetry you are talking about?

Line 208: get rid of an extra space in the phrase.

Same line: “structure parameter of temperature” by structure parameter of temperature turbulence (refractive index in the case of CN2).

Line 210-212: here the authors mentions very cursory a very important problem which is the variation of Cn2 because of the beam height variation across the landscape. It seems this is one point you should be more cautious in bring some references and eventually limit your study on the basis of this sensitivity parameter.

Line 213: only sensitive to temperatures. Add a period in the phrase.

Eq. [1] you introduce here an approximation that then you’ll use as an equality. Please explain and substantiate or directly correct the equation. Also, I wonder how much beta introduce error, in this case, a semi-arid environment.

Line 217: iterative methods have intrinsic convergence and resolution errors. You have to specify the convergence error and also how the average of Cn2 gives you a signal with enough SNR to keep the specific convergence factor. Now recently analytical methods have been developed that integrate the set of nonlinear equations in this casa Tatarskii and Monin-Obukhov similarity hypothesis set. See Gruber and Fochesatto, (2013).

Gruber M. A. and G. J. Fochesatto. 2013: “A New Sensitivity Analysis and Solution Method for Scintillometer Measurements of Area-Average Turbulent Fluxes” *Boundary-Layer Meteorology*, 149:65–83 DOI 10.1007/s10546-013-9835-9

Line 220: Zlas is a function where is that?

Andreas parameterization might not be valid for your site.- Can you justify here?

Zv: is the average canopy height but weighted by the extension of the plots?

Eq. [4] contains u* but it is not clarified here from where this is taken.

Here we can conclude that XLAS ONLY measures T* as a large-scale area-average variable but u* is a local variable or at least a variable measured at the scale of the EC system which is not the same as the XLAS. Explain please?

Line 225: rho is the air density and cp here are considered constants. Do they vary across the experiment?

Line 227: nomenclature is Number[space]unit. please correct all the way your text.

Line 228: change "circa" by "near". The correct use of "circa" in English is to indicate something that happened in the past (circa, 1000 AD) for example.

Line 230: how many "aberrant" values you have in the entire dataset. Please give more precision to the signal processing so that researchers can compare their work with yours in the future.

Line 247: and also gives the major sensitivity to H. See also (Gruber et al., 2014) for the specific analytic derivation of the sensitivity to the topography height.

Gruber, M. A., G.J. Fochesatto, O.K. Hartogensis, and M. Lysy. 2014: "Functional derivatives applied to error propagation of uncertainties in topography to large-aperture scintillometer-derived heat fluxes". *Atmos. Meas. Tech*., 7, 2361-2371, doi:10.5194/amt-7-2361-2014, 2014.

Equations 7 and 8: assume closure of energy balance at 100% please explain how this is possible. And what are your assumptions that lead to this approximation and what is the uncertainty in this assumption.

Line 271: Here the authors give an estimation of G/Rn energy partition that is known to be variable not only across a given landscape but also across landscapes. This needs to be carefully estimated. This goes from 31% to very low values in dense canopies. Please be more specific and give values of this factors across all your landscapes.

Line 284: change "meteo" by "meteorological station".

Lines 280-290: Here the authors bring parameterizations of G. And certainly it is appreciated this compilation. However, it would be best to have a discussion of how one of these parameterization is or may result more optimal for this work. It seems all the formulas were found and then tossed in this article to see what happens. –So compare your environment with the environment in which those parameterizations were developed and then decide or make some arguments about how to best use or adapt any of these parameterizations.

Line 294: basically with the current satellite technology we cannot estimate diurnal cycles. However, you must know that at higher latitudes Aqua and Terra have at least six-passages a day.

Line 300: I don't understand why the authors propose a=1 and b=0 and then find motivation on

finding that actually these are not zero. The approximation of Rn by SW (Short Wave Downwelling) is known in micrometeorology and only works to some extent in clear skies when Rn is dominated by SW downwelling. I mean Rn can be negative but never SWdown. So, the way this paragraph is written possess a problem since it is not physically correct.

Line 304: How you weigh the 10x8 km images data by the footprint? What kind of functions are used here to compute the footprint. Please explain.

Line 310: replace the “temperature of soil” by “soil temperature”.

Here you mention a “reference height” and simultaneously we are talking about a heterogeneous canopy and soil and canopy. Where is that reference height? And what are the assumptions and approximations you are taking by taking this assumption. For example, you are considering some variables at soil level but others at canopy level. How the reference height represents both? And what are the assumptions in terms of physical processes?

Eq. [15] you have here a radiative balance equation where it is assumed (without indication) that emissivity (on the left hand side) is =1. Also this equation needs a reference level and a specific condition for the fluxes to be added and represented at the reference level. Please make sure you are accounting for all these so that the reader can fully understand what your assumptions are and where and under what conditions your analysis is valid.

Line 319-320: is SPARSE better than TSEB? Can you give a little bit more explanation here? TSEB has modes to trait vegetation ALEXI and DIS-ALEXI. Are you saying that by incorporating aerodynamic functions makes SPARSE better than TSEB? Please clarify here what’s the extent and implication of your comment on the paper.

Line 325: from where you got the 30W/m2 minimum value? In some environments this will be three times G. Please justify this value.

Line 334:335: Here we need to be more specific. What data is from bibliography and what data comes from RS? Please be specific.

Line 343: Why you define an acronym MRT that is not used anymore? Acronyms that are not mentioned in the text anymore are unnecessary.

Line 343-347: this phrase is too long and badly constructed.

Line 349: We need more detail here. How many days or cases have been excluded from the entire dataset. We need to know how critical is this problem. Because if it is critical then it renders the method useless.

Line 355: k1.15 need space.

Line 357: explain clump-LAI measurements.

Delete the word “Bibliography” from Table 1. That column is for sources and a journal peer review is a source.

Line 379: “overpasses”

Line 383: The second step need a more substance. How come you are running a 30 min fluxes based on a single TIR input? This will result in diurnal cycle of fluxes that are totally biased. I would say that this approximation is only valid for time-intervals in which the turbulence conditions are not too different form the TIR observations.

Line 396: please revise the following wording “…complementary part to 1…”

Section 4.2 seems to go around and around the subject without going down to the specifics. I think is necessary to simplify the description of methods.

Line 407: how you define the wet conditions here? Rain through the day, a specific amount of mm? please be more specific here.

Eq. [21] assume 100% energy balance closure. You need to justify the use of this condition.

Line 429: “deduce” instead of “deduct”.

Fig. 5. This figure is a very low quality without precision in the axis. Also we see only RS data here while it is announced XLAS data.

Line 475: “convolving” Convolution has a very specific meaning in mathematics. Please verify the use of this term here.

Same for the use of modelled or modeled. Both expressions are fine however if your choice is to use words in British English (in this case modelled) you have to be consistent all the way through your paper.

Line 477: “dots”? seriously?

Line 478: Why these two days? Please give the reasons why you are specifically using those days. This is important because when scientist reading your paper would like to reproduce your results they will find no framework to produce such comparisons.

Figure 6. I don’t understand the coordinates (Y-axis and X-axis). Also the contours of XLAS footprint have no indications.

Line 482: what you mean by “hot pixel”? Please avoid jargon in the writing.

Line 489: In general models are calibrated based on EC systems and thus the deduced large-scale area-average fluxes derived from satellite remote sensing is controlled by LAS observations.

Line 490-500: In general, as the heterogeneity in vegetation, soil and eventually in topography leading to variables flows increases the divergence increases. There though cases in which even EC systems that are placed together at distance shorter than the convective ABL development verify more than 50/m2 differences (Starkenburg et al, 2015). So then results expressed here are within the range of reasonable values.

The only one physical explanation why the LAS path by being longer would give different results is when the heterogeneity is such that the BL that develops integrates patches of different thermodynamic and turbulent properties. Then, the mention of issue is interesting but without a correct explanation is useless.

Figure 7. contains features that are important to discuss since there is a change in the bias as function of the flux level. I wonder the authors to discuss this aspect from the physical aspects of the processes dominating this scale integration.

Figure 10. display several cases where there is a huge divergence in stress index particularly in April and July for both spacecraft.

Line 562: here the authors mentioned –uncertainties- but at no point in the paper we are discussing about this. As previously mentioned uncertainties come not only in EC and XLAS observations but also in the approximation used based on 100% closure in the energy balance. It is confusing and not clear definitively.

Line 565-570: give some explanation but actually is a description of the time-series. Can you provide a real-actual-explanation about what is the physical processes underlining this divergences and convergences.

Same from 570 to 575

Line 588: is this the actual explanation of why there is such divergence or is this another speculation?

Line 590-592: the error indicated here is extremely low now can you please indicate all-conditions in which this is valid and please circumvent this result to the specific interval of conditions in which this is actually valid.

Figure 11. From where and how you got errorbars in blue trace? Figure caption is not clear. We need a accurate description of the contents in the figure.

Line 610: “valorize” I wonder what the authors wanted to indicate here?

SVAT seems not to have been defined earlier.