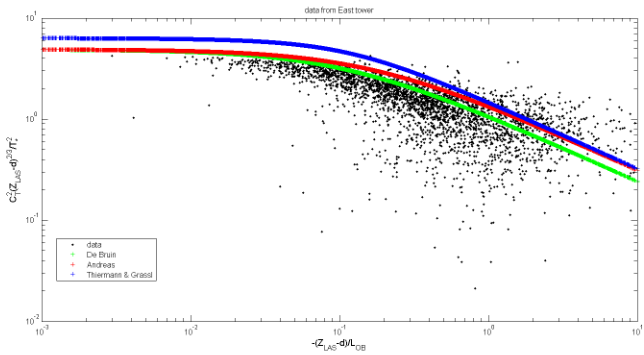
Authors Response to Reviewer 2 comments

	General comments	Authors response
1	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.	All figures will be improved in the revised version of the article. Particular interest is given to clarify axis ticks, grid and labels.
2	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.	Figures captions and legends will be enhanced in the revised version of the article in order to provide complete information.
3	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?	This article deals with an assessment of SPARSE model accuracy and operational use in a semi arid context over a heterogeneous landscape; the theoretical framework of SPARSE is only summarized since it has been detailed in (Boulet et al., 2015) as well as in the online documentation (http://tully.ups-tlse.fr/gilles.boulet/sparse); since it is critical to have a self-understandable methodology section in the revised version of this article, we will extend the explanation of the SPARSE methodology and add a diagram showing the flowchart of the SPARSE algorithm. There is no iteration till convergence in the SPARSE algorithm, only a decision tree with decisions made upon the sign of the retrieved soil latent heat flux component in case of invalidity of the unstressed vegetation initialisation. This will be detailed in the added figure showing the SPARSE algorithm. The 30W.m^2 is not a threshold to start iteration since there is not a convergence in SPARSE model but it is an arbitrary minimum positive value of soil latent heat flux (LEs) used

	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.	as the threshold for vegetation stress detection instead of 0, in order to take into account the contribution of vapour transfer from within the topsoil porous network as shown in (Boulet et al., 1997).
4	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.	In our study area topography is flat, and landscape is heterogeneous only from an agronomic point of view since we find different land uses (cereals, vegetables and fruit trees mainly olive trees with considerable spacing of bare soil); however, this heterogeneity in landscape features at field scale is randomly distributed and there is no drastic change in height and density of the vegetation at the scale of the XLAS transect (i.e. little heterogeneity at the km scale, most MODIS pixels have similar NDVI values for instance).Support for the MOST theory was assessed by looking at non-dimensional diagrams of normalized Ct^2 as the one below which will be shown in the revision as supplementary material. In this figure, we can see that most points are aligned on the theoretical curve of De Bruin et al. (1993). On that basis, we believe that MOST is valid. Points too far from the theoretical line will be excluded from our analysis. Also, on the basis of that figure, the Andreas parameterization will be replaced by the De Bruin one. We also have postprocessing selection criteria for the XLAS data, for instance, we select only H values above 50 W m^{-2} and we will analyse further the XLAS and footprint data on the basis of the z/L values. 
5	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	Uncertainties concern mainly: i/ the instantaneous remote sensing data: there is indeed an issue with the MODIS pixel heterogeneity and notably the distribution of components at the intersection between the square pixel and the XLAS footprint. Also, MODIS products, and mainly LST which is paramount in stress coefficient computation, are assumed to be reliable since we do not have means to reprocess them; however, results

	been computed. A reference is provided so that the authors can check on that. 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.	could be checked using Landsat high resolution TIR data. ii/ half hourly forcing and XLAS data (meteorological and flux data); iii/ the extrapolation method from instantaneous to daily results ; iv) unlike temperate areas in which sensible hat flux H is relatively low, in our semi-arid study area, H is mostly high leading to important difference between H and LE (which approaches zero) requiring more data post check . v/ the empirical estimation methods of soil heat flux G (3 methods were tested) as well as the possible daily heat accumulation can lead to possible errors in available energy estimation and in turn in residual LE estimation , hence, both minimum and maximum daily observed LE were presented, the same for the modeled daily LE presented by error bars. Despite all these possible uncertainties sources, our findings are reasonable compared to previous published results (SAMIR model,(Saadi et al., 2015)). Thank you for this interesting reference on which we draw on to add a paragraph in the revised version discussing the uncertainties in heterogeneous terrain based on pure XLAS observations.
6	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.	There are two EC stations located at the top of the towers (on the side of the XLAS emitter and receiver, resp.), which are used to process the XLAS data (initialization of u* and L) and one EC station in the ground, this will be detailed in the revised manuscript: i) The ground EC station, referred as the Ben Salem flux station measures convective fluxes exchanged between the surface and the atmosphere (H and LE) combined with measurements of the net radiation Rn and the soil heat flux G. Measured Rn and G were among the input data of XLAS derived sensible and latent heat flux computation. In addition measured Rn and G as well as measured H were used to calibrate the extrapolation relationship of the available energy and the sensible heat flux, respectively (sect. 3.3.2 and 4.2.3). ii) EC set-ups positioned on the two water tower top

		platforms were used to initialise friction velocity u* values in the scintillometer derived flux computation. These details will be added in the article revised version in "Experimental Setup" section.
7	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.	In the revised version, more physically-based explanation dealing mainly with the outliers will be added to describe the final figures.
8	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.	

	Comments in details	Authors response
1	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.	References will be put in chronologic order in the revised version
2	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.	The paragraph below will be added in the revised version: *"Indeed, the Mediterranean region is one of the most prominent "Hot-Spots" in future climate change projections (Giorgi and Lionello, 2008) due to an expected larger warming than the global average and to a pronounced increase in precipitation inter-annual variability. The major part of the southern Mediterranean countries, amongst others Tunisia, already suffer from water scarcity, and show a growing water deficit, due to the combined effect of the increasing water demand , and the limited and variable available resources (temporary drought and/or climate change)."*
3	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.	"*greatest*" is replaced by "*the larger*" in the revised version.
4	Line 56: I'll add complexity in. 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.	We have already mentioned the impact of land cover heterogeneity at large scale on the land atmosphere exchange: *"Moreover, at these scales, land cover is usually heterogeneous and this affects the land-atmosphere exchanges of heat, water and other constituents (Giorgi and Avissar, 1997)."* However, to develop this idea further, in the revised version, we will provide some more explanation about the hydro-meteorological processes complexity and its impact on climate variables: *"it is much more difficult at larger scales (irrigated perimeter or watershed) due to the complexity not only of the hydrological processes (Minacapilli and Ciraolo, 2007) but also of the hydro-meteorological processes. Indeed, at landscape scale, surface heterogeneity influences*

		regional and local climate inducing for example cloudiness and precipitation and temperature patterns difference at areas of higher elevation (hills and mountains surrounding the Kairouan plain) than at the lower elevation plain. Moreover, the land-atmosphere exchanges of heat, water and other constituent are affected by heterogeneous land cover (Giorgi and Avissar, 1997).”
5	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.	Remote sensing (RS) can provide estimates of large area fluxes in remote locations, but those estimates are based on the spatial and temporal scales of the measuring systems and thus vary one from another. Hence, one solution is to upscale local micrometeorological measurements to larger spatial scales in order to acquire an optimum representation of land-atmosphere interactions (Samain et al., 2012). However, such up scaling process is not always possible and results are not reliable in comparison to the RS distributed data. In order to keep the introduction as short as possible, we will only point out in the revised version one or two examples of complex physically based LSMs using RS data as inputs to derive ET.
6	Line 63: vegetation physical properties or characteristics?	In the revised version: “*vegetation’s physical properties*” will be replaced by “*vegetation’s physical characteristics*”
7	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	We agree with Reviewer 2 and the word “plot” induces ambiguity. “*plot*” will be replaced by “*field*” in the revised version.

	the abstract).	
8	Line 87: please rephrase the text between parenthesis.	In the revised version: *"(mostly derived from, say, actual water content in the root zone, wilting point and field capacity) "* will be replaced by: "*'mostly derived from the soil moisture characteristics i.e actual available water content in the root zone, wilting point and field capacity)"*
9	Line 93: Spell out FAO. If it is not being used anymore in the text, then no need to define an acronym.	In the revised version: *"FAO guidelines"* will be replaced by *"Food and Agriculture Organization-FAO guidelines"*
10	Line 98-99: get rid of parenthesis here. What is inside is part of the phrase.	Parentheses will be removed in the revised version.
11	Line 102: FAO-56 put a reference here. Or make a short phrase explanation.	The Allen et al. (1998) reference will be added in the revised version.
12	Line 103: what is "dry down"? please make sure you check consistency in all phrases.	*"Dry-down period is the period after rain or irrigation where the soil moisture is decreasing due to evapotranspiration and drainage. It is of great interest, because soil moisture has such a strong effect on nearly every aspect of the land surface (heat distribution, albedo, carbon uptake… etc.)."* This short explanation will be added to the revised version.
13	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.	Quotes will be removed for *single-source models* and *dual-source models*
14	Line 117: comma missing before etc.	It will be rectified in the revised version.
15	Line 128: add "they provide area-averaged sensible heat flux"	"*average sensible heat estimates*" will be replaced by "*area-averaged sensible heat estimates*" in the revised version
16	Line 130-131: incomplete phrase. And, can you elaborate a little bit more here?	This phrase will be rectified in the revised version as follows: "*Scintillometry can provide sensible heat using different wavelengths (optical wavelength and microwave wavelength ranges), aperture sizes (15-30 cm) and configurations (long-path and*

		short-path scintillometry)"
17	Line 132: delete space before comma.	It will be in the revised version.
18	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.	Indeed, the issue of the representativity of the heterogeneity (land use and irrigation practice) at the intersection between the MODIS pixels condidered as homogeneous and the XLAS footprint was not discussed in the submitted version of the article. We will add the suggested reference and discuss the relative percentages of Land Use classes within each MODIS pixel to provide a first guess on these relative heterogeneities.
19	Line 140: **large-scale area-average** this is the proper measurement that one obtains from a scintillometer.	In the revised version: *"Since the scintillometer only provides spatially averaged sensible heat flux (…)"* will be replaced by *"Since the scintillometer only provides large-scale area-average sensible heat flux (…)"*
20	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.	Please see authors' response to the general comment N°4.

21	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.	Indeed “*layer*” is the name of the approach, hence, the quote are removed in the revised version. More details about this approach is given in (Boulet et al., 2015)
21	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?	The resistance scheme is detailed in Boulet et al. (2015) and is similar to that used in Kustas and Norman (1999), cf. the (Monteith and Unsworth, 2007). V is either a temperature difference (soil-aerodynamic level or vegetation-aerodynamic level) or the corresponding vapour pressure difference. I is the flux component (sensible or latent) and R is the resistance to transfer (aerodynamic resistances within and above the vegetation, stomatal resistance). There is no need of specifying a soil resistance to evaporation because the evaporation rate is directly retrieved. The Series description of the electrical analogy used here is that of most LSMs following (Shuttleworth and Wallace, 1985) which describes the interactions within the soil-plant-atmosphere interface for sparse crops. The radiation interception by sparse crops might be difficult to represent with a layer approach, this will be further commented in the text.
22	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.	In the revised version: *“The main objective of this paper is to compare H and LE obtained using the SPARSE model and XLAS (...)”* will be replaced by: *“The main objective of this paper is to compare respectively modeled H and LE obtained using the SPARSE model and XLAS measured H and XLAS*

		derived LE (...)"
23	Line 158: put "(" to indicate the reference the cultivars are within the phrase.	This will be rectified in the revised version
	Line 173: what "double device" means for you. Please be specific.	This phrase will be simplified in the revised version and "*double device*" will be removed.
24	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.	Green contours are half-hourly XLAS footprints for selected typical wind conditions. High resolution SPOT5 image of 9th April 2013 was only used as background image to illustrate the land cover under the XLAS transect. Hence, figure 2 caption will be rectified in the revised version as follows: *"XLAS Set-up (XLAS transect (white), emitter and receiver are located at the extremity of each white arrow and half-hourly XLAS footprint for selected typical wind conditions (green), MODIS grid (black), orchards (blue) and the location of the Ben Salem meteorological and flux stations. This figure illustrates three colour (red, green, blue) composite of SPOT5 bands 3 (NIR), 2 (VIS-red) and 1(VIS-green) acquired on 9th April 2013 and showing in red the cereal plots".* On the hand, EC station flux measurements are not compared to XLAS fluxes along the article. This EC station utility has been already explained in the above responses (general comment N°6).
25	Line 196: I would write Extra Large Aperture Scintillometer (XLAS)	This will be rectified in the revised version
26	Line 198: Phrase: "Scintillometer is based on the scintillation method" what is this?	This will be rectified in the revised version
27	Line 198-200: What is the cause and what is the effect? This phrase is wrong please think about a little bit.	This will be rectified in the revised version as follows: *"Fluxes of sensible heat and momentum cause atmospheric turbulence close to the ground, and create, with surface evaporation, refractive index fluctuations due mainly to air temperature and humidity fluctuations (Hill et al., 1980)."*
28	Line 205: replace "bean" by "beam"	This will be rectified in the revised version
29	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	(Tatarskii, 1961) reference is added to the revised version

	references.	
30	Line 206: symmetrical to what? What is that symmetry you are talking about?	This sentence will be corrected: "... follows a bell-shape curve. This means that the measured flux is more sensitive to sources located towards the center of the transect than those close to the extremeties. As transmitter and receiver apertures are equal, the sensitivity is symmetrical with respect to that center and decreases similarly towards both ends"
31	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).	This will be corrected.
32	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.	The terrain is very flat; therefore there is little beam height variation across the landscape, except for what is induced by the different roughness of the individual fields. Since the interspace between trees is large, the effective roughness of the orchard is not significantly different from that of cereal fields, esp. given the measurement height.
33	Line 213: only sensitive to temperatures. Add a period in the phrase.	This will be corrected.
34	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.	This will be corrected; an equality sign will be used in Eq. 1. The sensible heat flux dominates the energy balance in most cases; therefore the Bowen ratio is mostly above one. The influence of the beta correction has been analysed in (Solignac et al., 2009) which shows that since the beta closure method does not rely on an exact locally observed beta it is far less sensitive to the precision on beta.
35	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).	This will be verified in the revised version.

	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	
36	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?	Z_{LAS} is not a function, since XLAS experiment is done over a flat surface, Z_{LAS} is the XLAS height, "*effective*" is removed because it induces confusion. We indeed test the De Bruin (De Bruin et al., 1993) parameterization in the revised version (cf. Figure above). Zv estimation method is detailed by the end of section 4.1. It accounts for the various heights within the footprint selected using angular zones from the center of the transect.
37	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?	u* is not taken from EC system it is computed based on an iteration approach in the beta closure method, only the initialization value of u* was taken from the EC station positioned on the western water tower .
37	Line 225: rho is the air density and cp here are considered constants. Do they vary across the experiment?	Indeed, air density, pressure and temperature depend on the location on the earth, on altitude and on the season of the year. However, in our study, standard values of air density (ρ) and air specific heat at constant pressure (cp) were used without verifying their variation across the experiment since our study concerns a limited extent (10km*8km, same earth location) with flat terrain (no altitude variation) and without a considerable temperature difference between the hot and cold seasons (average monthly temperature oscillates between 10°C and 28°C).
38	Line 227: nomenclature is Number[space]unit. please correct all the way your text.	This will be rectified in the revised version
39	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.	This will be rectified in the revised version

40	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.	The following paragraph will be added to the revised version: *Furthermore, half hourly H_XLAS aberrant values (measurement errors and values higher than 400 w.m^{-2} arising from measurement saturation) were ruled out (3% of the total half hourly measurement throughout the experiment duration). And then daily H_XLAS was computed as the average of the half hourly H_XLAS, 9% of the daily aberrant values were ruled out following the same selection criterion as the half hourly measurement."*
41	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.	Again, the terrain here is very flat and does not induce any disturbance linked to topography.
42	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.	Please see authors' response to the general comment N°4.
43	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.	Indeed G estimation was the most uncertain variable in this study, and that's why we tested three methods to compute it since based on in situ data, we generally found a G accumulation and the daily G is rarely zero. This part will be largely discussed in the revised version.
44	Line 284: change "meteo" by "meteorological station".	This will be rectified in the revised version
45	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	We used standard relationships used in models such as SEBS (Su et al., 2001). An overview of the validity of the relationship for the sole Ben Salem EC station (cereal) will be illustrated in the

	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.	revision.
46	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.	We agree with Reviewer 2.
47	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.	This paragraph as well as the associated result section (6.1) will be rephrased in the revised version. Indeed, the extrapolation from an instantaneous flux estimate to a daytime flux assumes that the surface energy budget is "self-preserving" i.e. the relative partitioning among components of the budget remains constant throughout the day. However, many studies (Brutsaert and Sugita, 1992;Gurney and Hsu, 1990;Sugita and Brutsaert, 1990) showed that the self-preservation method gives day- time latent heat estimates that are smaller than observed values by 5-10%. Moreover, (Anderson et al., 1997) found that the evaporative fraction computed from instantaneous measured fluxes tends to underestimate the daytime average by about 10%, hence, corrected parameterization was used and a coefficient=1.1 was applied. Similarly, (Delogu et al., 2012) founded an overestimation of about 10% between estimated and measured daily component of the available energy thus, a coefficient =0.9 was applied. The (Delogu et al., 2012) corrected parameterization were tested, since, in our study case also an overestimation between estimated and measured AE was found, but this coefficient did not give consistent results, therefore, we had to calibrate the extrapolation relationship in order to get accurate daily results of AE and H. Thereby, the applied extrapolation method was tested using in situ Ben Salem flux station

		measurements. Indeed, daily measured AE (all the same for H) computed as the sum of half-hourly measured AE, was compared to daily AE computed using the extrapolation method from instantaneous measured AE at Terra (equation 13) and Aqua (equation 14) over pas time. This comparison gave an overestimation of about 15% (for AE), hence, corrected parameterizations of available energy AE (coefficients summarized in Table 2) were applied to remove the bias between measured and computed AE.
48	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.	Daily footprints were computed as a weighted sum of the half hourly footprints by the XLAS sensible heat flux. Weighing the 10x8 km images data by the footprint means multiplying the 10x8 km result grid by the footprint (weight coefficients ranging from zero and one).
49	Line 310: replace the "temperature of soil" by "soil temperature".	This will be rectified in the revised version
50	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?	Reference height here is the measurement height of the meteorological forcing (2.32 m). This will be précised in the revision.
51	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.	Details will be added to the revised version
52	Line 319-320: is SPARSE better than TSEB? Can you give a little bit more explanation here? TSEB has modes to trait	A detailed intercomparison study between TSEB and SPARSE based on several flux stations is underway, first results indicate that bounding the

	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.	fluxes simulated by both models by the potential rates given by SPARSE improves the performance of both models which have otherwise similar performances, though constrasted for the various cover types. In SPARSE the aerodynamic functions are those used in almost all Land Surface Models. ALEXI and DIS-ALEXI rely on coarse scale (few km) MSG data, and intercomparison of the ALEXI ET product and the scintillometer will also be carried out in the next future.
53	Line 325: from where you got the 30W/m2 minimum value? In some environments this will be three times G. Please justify this value.	Please see authors' response to the general comment N°3.
54	Line 334:335: Here we need to be more specific. What data is from bibliography and what data comes from RS? Please be specific.	After this sentence, bibliography, remote sensing and in situ data were detailed in the following paragraphs, however, in order to be more clear, this section will be rephrased in the revised version.
55	Line 343: Why you define an acronym MRT that is not used anymore? Acronyms that are not mentioned in the text anymore are unnecessary.	Rectified in the revised version
56	Line 343-347: this phrase is too long and badly constructed.	This paragraph is reworded in the revised version.
57	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.	360 daily data were excluded from the total daily data (1033 days), the following sentence is inserted in the revised version: *"(...) hence, days with missing data in MODIS pixels regarding the scintillometer footprint (35% of the acquired data) were excluded"*
58	Line 355: k1.15 need space.	This will be rectified in the revised version
59	Line 357: explain clump-LAI measurements.	Clump LAI is the value of the LAI of an isolated element of vegetation (tree, shrub...); if this element occupies a fraction cover f and is surrounded by bare soi, then the clum LAI value is simply equal to the area average LAI divided by f. This will be specified in the revised version.
60	Delete the word "Bibliography" from	This will be rectified in the revised version

	Table 1. That column is for sources and a journal peer review is a source.	
61	Line 379: "overpasses"	This will be rectified in the revised version
62	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.	Indeed, the SPARSE model was run at a half hourly time step using the half hourly meteorological measurements ; assuming that the either the stress factor or the evaporative fraction are invariant during the same day, the diurnal modelled fluxes are accounted for by recovering the diurnal course of either potential ET or available energy. Running the SPARSE model at half hourly time step is only done to get half hourly latent heat flux at potential conditions LEpot wich is equivalent to a reference evapotranspiration whose calculation depends only on half hourly climatic data. This LEpot is used later when computing daily LE based on the stress factor method (section 4.2). This will be better expalined and more detailed in the revised version.
63	Line 396: please revise the following wording "…complementary part to 1…"	This will be rephrased in the revised version.
64	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.	This will be rephrased in the revised version.
65	Line 407: how you define the wet conditions here? Rain through the day, a specific amount of mm? please be more specific here.	Wet conditions are defined on the basis of a significant amount of rain recorded in the previous day (more than 5 mm).
66	Eq. [21] assume 100% energy balance closure. You need to justify the use of this condition.	Please see authors' response to the general comment N°4.
67	Line 429: "deduce" instead of "deduct".	This will be rectified in the revised version
68	Fig. 5. This figure is a very low quality without precision in the axis. Also we see	Please see authors' response to the general comment N°1.

	only RS data here while it is announced XLAS data.	
69	Line 475: "convolving" Convolution has a very specific meaning in mathematics. Please verify the use of this term here.	In the revised version: *"By convolving the XLAS footprint with the SPARSE derived H, we were able to compare compare the modelled values (H_SPARSE t-FP) with the XLAS measurements (H_XLAS t)."* will be replaced by "*SPARSE derived H was weighted by the XLAS footprint in order to be able to compare the modelled values (H_SPARSE t-FP) with the XLAS measurements (H_XLAS t)"*
70	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.	This will be rectified in the revised version
71	Line 477: "dots"? seriously?	This will be rectified in the revised version
72	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.	Selection criteria will be added to the revised version: - Day 2013-86 (24 March 2013) is in the cold season and day 185-2014 (4th July 2014) is in the warm season in order to highlight the land cover impact on LST and thus on modelled H (trees and rainfed and irrigated cereals in winter vs. only irrigated trees and vegetables in summer). - Day 2013-86 (24 March 2013) shows footprint of strong south wind wile the footprint of day 185-2014 is of a light north wind
73	Figure 6. I don't understand the coordinates (Y-axis and X-axis). Also the contours of XLAS footprint have no indications.	Figure 6 as well as its caption will be improved in the revised version
74	Line 482: what you mean by "hot pixel"? Please avoid jargon in the writing.	Hot pixel systematically means a pixel with high LST and low NDVI. A short explanation will be added to the revised version.
75	Line 489: In general models are calibrated based on EC systems and thus the deduced large-scale area-average fluxes derived	Indeed in this study, SPARSE model was run in an operational way at landscape scale without parameters calibration, since in our study area, we do not have EC station for each crop type.

	from satellite remote sensing is controlled by LAS observations.	However, SPARSE results at field scale were already compared to EC measurement in an irrigated wheat field and a rainfed wheat field in (Boulet et al., 2015)
76	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.	This part will be improved in the revised version based on this comment.
77	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.	This part will be improved in the revised version. Indeed, possible explanations are: - the XLAS measurement saturation; according to the "Kipp & Zonen LAS and XLAS instruction manual", for a path length of 4km and a scintillometer height of 20 m, saturation measurement problem starts from H values of about 300 W.m^{-2} - Uncertainties on the correction of stability using the universal stability function - Potential inconsistencies between the area average MODIS radiative temperature and the air temperature measured locally at the meteorological station.
78	Figure 10. display several cases where there is a huge divergence in stress index particularly in April and July for both spacecraft.	These individual dates will be discussed in the revised version.
79	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%	Please see authors' response to the general comment N°4.

	closure in the energy balance. It is confusing and not clear definitively.	
80	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.	The discussion part relating to Figure 11 will be improved in the revised version.
81	Same from 570 to 575	Same as comment 80.
82	Line 588: is this the actual explanation of why there is such divergence or is this another speculation?	Same as comment 80.
83	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.	Same as comment 80.
84	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.	Figure 11 caption is improved in the revised version. Error bars for the SPARSE results show the minimum and the maximum daily evapotranspiration (ET) resulting from the three methods used to compute daily ET from instantaneous modelled ET at the time of Terra and Aqua overpasses: evaporative fraction, stress factor and residual methods, hence, six estimates of the daily modelled ET are produced. This will be mentioned in the caption.
85	Line 610: "valorize" I wonder what the authors wanted to indicate here?	- This word is rather vague indeed, we will precise the perspectives of this work, notably using a LSM applied at the field scale (Etchanchu et al., 2017) to analyse the scaling properties from the field to the footprint of the XLAS and the MODIS pixels similarly to the reference provided by Reviewer 2 (Bai et al., 2015).
86	SVAT seems not to have been defined earlier.	This will be rectified in the revised version.

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