Hydrol. Earth Syst. Sci. Discuss., 11, C70–C86, 2014
www.hydrol-earth-syst-sci-discuss.net/11/C70/2014/

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Evaluating the SSEBop approach for evapotranspiration mapping with landsat data using lysimetric observations in the semi-arid Texas High Plains" *by* G. B. Senay et al.

G. B. Senay et al.

senay@usgs.gov

Received and published: 6 February 2014

Responses to Dr. George Paul's Comments (same comment is attached as a supplement in pdf)

We thank Dr. George Paul for providing his comments on this manuscript. Below is our response to his critical review comments and unwarranted remarks.

As a background, we would like to inform the reviewers/readers that Dr. Paul had reviewed this manuscript as part of the internal review process of USGS and USDA. His overall impressions in his previous review were positive and his comments were

sound and professional. We addressed and incorporated most of his comments on the revised manuscript before its publication on HESSD.

In his comments that were made on December 16, 2013 as part of internal review process, Dr. Paul recommended the publication of the manuscript by stating the following: "…the study could form a valuable reference for future researchers attempting to develop simple thermal remote sensing based algorithms to estimate ET…".

However, we are completely surprised by the tone of Dr. Paul's current "short" review comments as it is dramatically different from his constructive comments in his original internal review remarks.

We would like to believe that Dr. Paul has a genuine interest in improving the scientific quality of the manuscript. Below are our detailed responses to Dr. Paul comments:

Comment 1: "Everything should be made as simple as possible, but not simpler". This profound statement by Albert Einstein fits aptly to the work presented in this manuscript. The manuscript presents a simple method to map actual evapotranspiration; however the approach used and analysis performed here breaks all laws of physics and logics.

Response: We believe that this conclusion by Dr. Paul is a result of his misunderstanding of the novel but simple ET method presented in this manuscript. However, it is not a surprising reaction as some of the alternative ET estimation methods available in the literature are full of mathematical details, but very complex to implement at a large scale operationally and demand inputs from weather datasets that are not available in some (most?) parts of the world.

On the other hand, against the "too simple" assertion by the reviewer, a supportive statement from Einstein states: "the grand aim of all science is to cover the greatest possible number of empirical facts by logical deductions from the smallest possible number of hypothesis or axioms".

We believe the SSEBop approach provides a useful and reliable estimate of ET with fewer parameters as is demonstrated in this manuscript and previously published papers.

Comment 2: To start with, let's take a look at Eq. (1) given in the manuscript ETa = ETf * k.ETo (1) where ETa is the actual evapotranspiration, ETf is the fraction and [k.ETo] is the conversion of grass reference ET into alfalfa reference ET (ETr). More common form of this equation is

ETa = kc * ETr (2)

In Eq. (2) kc is the crop coefficient which is equivalent to ETf in Eq. (1). The calculation of ETr is standardized, thus ultimately the accuracy of ETa would depend upon how accurately kc or ETf is computed. An interesting property of Eq. (1) and one on which several algorithms bank on is the fact that ETr sets the upper limit (or boundary condition) and then it all depends on how nicely the scaling is done (using kc) depending on the landuse/landcover. With this brief background, several very specific issues are being raised on this study.

Response: Dr. Paul did not point out a problem with physics or logic until this point. The more common formulation proposed by the reviewer is incomplete since he did not specify whether ETr is based on grass reference or alfalfa reference crop. We use used "k.ETo" since we defined ETo to be grass reference crop.

Comment 3: 1) The formulation of Eq. (3) in the manuscript. The bulk formulation of sensible heat (H) based on the flux gradient relation is given as:

H = row*Cp*(To-Ta)/rah (3)

here rah (sm-1) is the aerodynamic resistance to heat transfer between the surface and the reference level, and Ta is the air temperature, To is defined as the extrapolation of Ta down to an effective height within the canopy at which the vegetation component of H and latent heat (LE) fluxes arise given by do+zoh. From the Monin-Obukhov (M-O)

similarity theory, the aerodynamic resistance (rah) is defined as the resistance from height zoh+do having an aerodynamic temperature, to the height zref. In Eq (3) To-Ta is defined as the temperature gradient dT. The author used the Eq. (3), however defined dT to his convenience as Th-Tc, where Th is hypothetical hot temperature and Tc is daily maximum temperature.

Response: There is a major misrepresentation of the manuscript in the above statement. Tc is not the daily maximum air temperature. It is rather the cold boundary condition that can be derived from the daily maximum air temperature. Without this key correction, the approach would not work at all and we can understand how the reviewer probably arrived at such conclusions. But considering the nit-pick nature of the review, it is surprising how the reviewer missed this key point.

Comment 4: A long discussion is not warranted to prove that dT as defined by the author is wrong; This would be like equating oranges with apples, simply because both are round.

Response: It not clear to us why dT cannot be defined as the difference between two temperatures (Th-Tc), unless there is a "patent" associated to the specific definition of the term. This is one of the reasons why the SSEBop model is labeled differently from other surface energy balance models, i.e., the approach including parameters are defined differently but still based on physical principles. Dr. Paul did not explain why/how the reformulation of dT violated physical principles except that it is not the same definition as other models. Note that dT in SSEBop is the difference temperature between hot and cold boundary temperatures when all energy is converted to sensible heat. His use of dT is for sensible heat calculation at all conditions including in the presence of latent heat flux.

The apple and orange comment remains an intriguing addition to Dr. Paul's confident dismissal of the manuscript.

Comment 5: Nowhere in the literature is Eq. (3) used for computation of daily H value;

for obvious reasons. Again a long explanation could be provided to prove that Eq. (3) cannot be used on a daily time step, however, we leave it with request to author to provide one reference with daily time step usage of Eq. (3). Further with remote sensing algorithms it would have been a revolution, just compute daily H and daily Rn then get LE as residual; getting rid of the complex instantaneous to daily interpolation problem. Refer Konda and Ishida, 1996 (Journal of the atmospheric sciences), Vol. 54, Sensible Heat Flux from the Earth's Surface under Natural Convective Conditions, for an explanation on why Eq. (3) cannot be used for daily time step.

Response: Again, this is a misunderstanding or misrepresentation of the SSEBop approach. The model does not solve for sensible heat and calculate LE as a residual. Instead, SSEBop calculates LE directly. How? Under-clear sky conditions where remote sensing ET from thermal-based approaches can be solved, we estimate/predefine dT for a given date and location. This dT represents the difference in surface temperature between wet/cold and dry/hot surfaces. Then ET is calculated as a fraction of reference ET depending on the magnitude of the Land Surface Temperature between Th and Tc, through linear interpolation.

Dr. Paul did not show/explain how this assumption (that a hypothetical dT can be estimated under-clear sky conditions) violated physical principles. Yes, because this approach is unique we don't have a literature to support this except our own previous work.

Comment 6: The author relates the bare soil sensible heat flux with net radiation. Uses the net radiation formulation to calculate daily net radiation; however they use an albedo value of 0.23 which is a recommended value for cropped surface. How do they explain using an albedo value for cropped surface for bare soil net radiation estimation?

Response: This is a fair criticism. But it is important to note how this albedo is used. It is used to estimate clear-sky net radiation over a bare surface which will be later used to estimate dT along with a calibration process in the estimation of the aerodynamic

resistance over bare surface.

Knowing how albedo of a dry bare soil varies widely by soil type (<0.18 to > 0.3), we kept it at 0.23 for the following reasons: 1) there is no authoritative source that recommend one value, 2) the exact value of bare soil albedo is not very important in the SSEBop formulation since an error in the clear-sky radiation due to the wrong use of albedo would be compensated in the estimation of the aerodynamic resistance term through the calibration process. Thus our use of 110 ms- for rah takes into account our albedo estimate at 0.23. Because of our interest in dT, an error in albedo would be compensated by the estimation of rah in the calibration process. For details of rah estimation please refer to Senay et al. (2013).

Comment 7: Page 729, line 10 (and Page 735 line 9) of the manuscript states that the dT value does not change from year to year. The formulation of dT (Eq. 3) involves computation of net radiation. How is it possible that net radiation for a specific location for a particular day is not changing from year to year, even with the clear-sky assumption? Clearly, net radiation computation uses weather parameters (temperature, vapor pressure, etc.) as inputs which vary for particular day from year to year.

Response: That is true, weather parameters such as minimum and maximum air temperature and actual vapor pressure are needed in the calculation of net long wave ration according to Allen et al. (1998). But we used climatological values since our interest is establishing boundary conditions. Besides, the relative contribution of long-wave radiation is much smaller in most vegetated surfaces where ET estimation is important as compared to the net short wave radiation that does not use weather parameters under clear sky conditions. We will clarify this point more clearly when we revise the manuscript.

We appreciate these comments, but we still fail to see where the physical laws or scientific logic were violated.

Comment 8; 2) In Eq. (1) the value of k was taken as 1.25, further stating (Pg. 728, Line 25) that this is a recommended value of k and provided the reference to Allen et al., 2011a. In another paper by the author (Senay et al., 2013) which has been heavily cited in this manuscript, the value of k is taken as 1.2, again stating that this is a recommended value. Further, in another paper by the author (Senay et al., 2011), he states that a ‘k’ value of 1.1 was determined for the Texas Panhandle (region under study in the present manuscript); which leaves the question on why 1.25 was taken in this study. The value of ‘k’ is beyond doubt a sensitive parameter, how then the author keeps changing it according to his convenience?

Response: The value k of 1.25 was used in this manuscript because we used the REF-ET calculator (Allen et al., 2011a) to calculate ETo from the Bushland weather data. As it has been shown by different ET models, the method used in calculating ETo matters and thus we wanted to maintain consistency with the recommended multiplier. It is true Senay et al (2013) recommended a 1.2 factor but also stated the need to evaluate this factor using a calibration/validation process since other parameters may have a compensating bias. In Gowda et al. (2009), the 1.1 coefficient was not a “k” value but a calibration parameter. It is clearly stated in that paper. While we agree this can confuse readers, Dr. Paul did not explain how this violates the physics and logic of ET modeling.

Comment 9: 3) Eq. (3) was used to compute dT (on daily time step) and further dT was defined as: dT = Th - Tc (4)

where, Tc is the daily maximum air temperature and Th is temperature computed from above equation (Eq. 4) and termed as hot temperature. Now the scaling factor used (ETf ; Eq. 2 in the manuscript) has Th and Tc as daily values whereas Ts is instantaneous surface temperature. The author is mixing up daily and instantaneous values to come up with a factor; logically its redibility is questionable.

Response: Dr. Paul’s comments stem from misunderstanding of the SSEBop methodology:

a) Tc is not daily maximum air temperature as explained before, it is the cold boundary LST, the equivalent of the “cold” pixel LST with other familiar approaches. Again, a fraction of the air temperature is used since there is a strong correlation between maximum air temperature and “instantaneous” cold pixel LST.

b) Since Tc is now “instantaneous”, by the above formulation, Th is also “instantaneous”. Thus, instantaneous Ts is being evaluated between two ‘instantaneous” boundary surface temperatures.

c) What appears to be a parameterization challenge (questionable credibility in logic) is because Dr. Paul failed to recognize Tc as the surrogate for the cold-pixel LST and probably mixing it up with the daily dT. Remember the daily dT is constant but not the daily Tc and Th. We ask Dr. Paul to carefully examine the formulation paper (Senay et al., 2013) with a fair and unbiased treatment, with the intent to understand!

d) Also, on the implied question of mixing a daily dT with instantaneous Tc and Th: Although the measurements of LST (Tc, Ts, and Th) are “instantaneous”, we argue that their expression at the time of satellite measurement includes an energy memory distribution from the previous day. The soil-vegetation complex has a memory for energy and water! This can be proven by comparing our daily predefined dT from the instantaneous differences calculated between the hot and cold pixel-values, which is in the order of 15-20 K during the growing season in most parts of the world. Otherwise, the method would fail.

e) So, we don’t see a violation of physics or logic, except a misunderstanding.

Comment 10: 4) In Eq. (4) Tc is the cold reference point. How did the authors decide to relate cold reference point with daily maximum air temperature; why not the daily average air temperature or daily minimum air temperature was used? In a semi-arid region, as in this study area the maximum air temperature reaches in the late evening (between 5pm and 6 pm), which again raises issues with the formulation of scaling factor developed.

Response: This is a fair question. Again, for operational application around the world, as a tool to monitor agricultural conditions especially in the economically developing world (for which the model was initially developed as part of the USAID famine early warning project), we were looking for readily available air temperature data. Most countries have monthly Tmin and Tmax or Tavg but not hourly air temperature data. Since the LST that we used here is acquired in a given day, we found it logical to experiment if the Tmax would correlate with the LST of a wet surface. Indeed, while all correlate well, the Tmax made the strongest correlation with LST of the cold pixel, thus Tc was formulated as a fraction of the Tmax of a given sensor through a calibration factor.

Comment 11: 5) No clarity on how the land surface temperature is retrieved. How did the author get the mean value for path radiance, narrow band thermal radiation and narrow band transmissivity of air? Did they have radiosonde data and ran MODTRAN to get these values. What are their values? 6) Page 731 line 11, the manuscript says SSEBop incorporated 'simple set of hybrid algorithm'. What does the author mean by hybrid algorithm? The author lists a standard procedure and adopted an equation (Eq. 5 in the manuscript) from METRIC; what is hybrid about this?

Response: The choice of the world hybrid maybe confusing. In this sense it was meant to refer to the mixed use of approaches from Jimenez-Munoz and Sobrino and Allen et al. We will clarify this in the revised version.

Comment 12: 7) Page 731 line 20. "Emissivity values were computed using NDVI-based algorithm proposed by Jimenez-Munoz and Sobrino, eliminating the need to use LAI to estimate emissivity". This sentence is completely useless and misleading. Surface emissivity can be computed using any of the biophysical parameter derived from remote sensing data and there exists numerous empirical models to do so. The sentence in the manuscript implies that LAI is the prevalent method to estimate emissivity and author found this new method to utilize NDVI instead; which is an atrocious claim.

Response: It is not clear how the reviewer saw this as an atrocious claim. There was no intended claim at all in this statement except to refer to the approaches we used through citations. We shall improve the statements to avoid such implications. Again, these are valid comments, albeit too harsh for the issues raised, but we still did not see the break in physical laws or scientific logic except probably poor choice of words.

Comment 13: 8) Page 731 Line 22. " corrected thermal radiance (Rc) is derived using algorithm given by Wukelic et al. using assumptions reported in allen et al. 2007". Again a misleading and useless sentence; what are the assumptions?

Response: The focus of this study was not in the estimation of LST. Therefore, we cited references so that interested readers can find out more information on the LST estimation method and underlying assumptions.

Comment 14: 9) The aggregation method used by the author is completely wrong and there is no reference provided for such type of analysis.

Response: Dr. Paul did not explain or show how aggregation of discrete time series data does not lead to improved statistics. To the contrary his worked out example reinforces the fact that an aggregation by summation or averaging reduces errors. Actually, that is what he found, i.e., RMSE of 13.8% compared to no aggregation at 20.3%.

The problem is that his conclusion is completely different from what was claimed in the manuscript. We did not claim that the order of aggregation in a time series data did not matter! This would be tantamount to saying the sum of January and February data or their errors should not be different from the sum of January and June data or errors.

Unless there is a major statistical concept Dr. Paul wants to share, from the central statistics theorem, we know that the mean will have a smaller variability than the individual data points, i.e, while the average variability of individual data points is expressed by the standard deviation, the average variability of the mean is actually expressed by standard deviation divided by square root of the sample size.

In this manuscript, we used the sum of consecutive data points, but a comparable result would have been obtained if we used the average of consecutive data points for our aggregation. The final result is to show that random errors tend to cancel out as we aggregate (average or sum) data points. But we did not claim that the order of aggregation does not matter. In a time series data such as ET where there is a strong serial auto-correlation, it is not clear why Dr. Paul concluded outside of our claim.

Again, it not clear where the physical laws were broken or the logic was flawed based on what was claimed. Dr. Paul did not comment on the formulation of the error analysis equations.

Comment 15: The author is aggregating discrete day's value and coming up with improved statistics. Below is worked example to prove that the analysis done is completely wrong. Let say there are six observed and modeled values of daily ET (mm/day) Table 1. Table 1: Example observed and modeled ET (mm/day) Sl. No., Observed, Modeled 1., 0.4, 0.1 2., 4.1, 2.7 3., 1.1, 0.4 4., 7.3, 6.4 5., 6.7, 7.7 6., 7.4, 8.2 The RMSE computed for Table 1 is 0.91 mm/day (20.3%). Now as per the author's approach aggregation is done at 2 day period by summing up two discrete days, by adding data points 1 and 2, 3 and 4, 5 and 6, the resultant is shown in table 2. Table 2: Aggregation of the two day from table 1. Observed, Modeled 4.5, 2.8 8.4, 6.8 14.1, 15.9 The RMSE for table 2 is 1.7 mm/day (18.9%). Now again an aggregation is performed for 2 day period from table 1 but this time the addition is done in a slightly different order as 2 and 3, 4 and 5, 6 and1. The resultant is shown in table 3 Table 3: Aggregation of the 2-day sum from table 1 in the order as specified in text. Observed, Modeled 5.2, 3.1 14.0, 14.1 7.8, 8.3 The RMSE for table 3 is 1.2 mm/day (13.8%). The point is clear here, the order of aggregating the data would govern the statistics.

Response: Again, we did not claim that the order of aggregation does not matter in the manuscript. We showed an approach to separate random vs bias errors. But Dr. Paul did not comment on the validity of the Equations. Even with his own calculations, the RMSE has been reduced but not increased. His paring of data 6 and 1 is equivalent to

paring Sep (end of season) and May (start of season) data points. That was not what we did in this manuscript. We aggregated data that made ET sense, i.e., consecutive data points such as May and June, July and Aug, but not Sep and May.

We don't believe it was in scope of the manuscript to address the differences between handling probabilistic (dealing with random data points where probably Dr. Paul expected similar results) and time-series datasets in the temporal aggregation section.

Comment 16: Again, a lengthy explanation is not warranted to prove the senseless analysis performed in the manuscript. The figures, tables, and discussions pertaining to the aggregation analysis are sham and should be seen seriously.

Response: We strongly disagree with Dr. Paul's abrasive conclusion that this is a "sham". To the contrary, it is a logical fallacy (a red herring, a non sequitur!) to use unrelated concepts such as "order of data" in order to discredit an honest effort.

Comment 17: Page 737 Line 10- There is a 7% difference in the RMSE between dryland and irrigated yet the author say there is no 'apparent difference'. How much difference is required to establish difference?

Response: This is a fair point. We will report the difference as is to avoid potential bias in interpretation.

Comment 18: Page 738 lines 7- 9: The author is saying that mean ETa value increased when aggregation is done. Isn't that very obvious? What kind of value does such sentence add?

Response: Sometimes the obvious needs to be stated to make explanations clearer. However, we agree improved rephrasing should improve the document.

Comment 19: Page 738 Line 25: The author says SSEBop's approach of using a linear assumption between hot and cold boundary condition is valid. In SEBAL, a linear relationship is clearly developed using the hot and the cold end member pixels. In this manuscript where is the linear relationship and what is the linear assumption between hot and cold boundary condition? Author is requested to clarify how they formulated/derived Eq. (2)

Response: This is the basic assumption in other surface energy balance models such as SEBAL and METRIC except that our dT is predefined and the “instantaneous” Tc and Th is tied to the fraction of the daily maximum air temperature. The linear proportional relationship between Ts and ETf is evident in Equation 2. The assumptions and formulations have been explained in several of the papers the reviewer referred to earlier. We will include more clarification in this in the revised manuscript. For example, in Senay et al (2007) we presented a justification for the SSEB formulation as follows: Although solving the full Energy Balance (EB)âĂŘbased approach has been shown to give good results, the data and skill requirements to solve various terms in the equation are prohibitive for operational applications. The SSEB approach estimates actual ET while maintaining and extending the major assumption in SEBAL (Bastiaanssen et al., 1998) and METRIC (Allen et al., 2007), whereby the aerodynamic temperature gradient between the land surface and air (nearâĂŘsurface temperature gradient) varies linearly with land surface temperature. This relationship is based on two anchor pixels known as the hot and cold pixels, representing bare dry agricultural fields and wellâĂŘvegetated wet fields, respectively. In SEBAL, at the cold (satellite) pixel, H is assumed negligible (i.e. Hcold = 0) and at the hot pixel, LE is set to zero which results in Hhot = (RnâĂŘG)hot. In METRIC, at the cold pixel, LE is based on a reference ET (Allen et al., 2007) using ground weather data from solving the PenmanâĂŘMonteith equation for tall (0.5 m) alfalfa and at the hot pixel, and LE is set to zero which results in Hhot = (RnâĂŘG)hot similar to SEBAL.

In SSEB, this assumption is extended where LE also varies linearly between the hot and cold pixels in proportion to the land surface temperature based on the logic that the temperature difference between soil surface and air are linearly related to soil water (Sadler et al., 2000). Furthermore, crop water balance models estimate actual ET using a linear reduction from the potential ET depending on the soil water (Allen et al.,

1998; Senay and Verdin, 2003). The SSEB approach can be compared with the Crop Water Stress Index (CWSI) (Jackson, 1982) derived from the temperature difference between the crop canopy and the air. Dividing the canopyâĂŘair temperature difference by the known upper and lower canopyâĂŘair temperature difference creates a ratio index varying between 0 and 1. The upper limit is reached when plant transpiration is zero, this occurs when water is not available in the root zone (Qiu et al., 1999) and the lower limit is reached when the crop transpires at full rate. In SSEB, surface temperature values of cold and hot pixels are equivalent to the lower and upper limiting canopy temperatures of the CWSI method.

Comment 20: Page 739 Line 13-19: Never seen a study where bias is calculated from a dataset and then apply the bias correction on the same dataset and conclude that the results improved.

Response: The study presentation separates the bias corrected (unbiased) from the biased (original) and the results were presented for comparison purposes only and not for using the bias-corrected model for prediction purposes. Due to limited data points (2 years) there was no attempt for separating model calibration and validation data sets and this was not the purpose of the study.

Comment 21: Page 736 Line 1: The author say close proximity of four field yield identical hot and cold boundary condition. The question here is, what is the distance required to see a difference between the hot and cold temperature (between fields).

Response: Sure, this depends on the complexity of the hydroclimatic conditions. We will include statements that show the uniformity of the climatic conditions in the lysimeter fields.

Comment 22: The statement made by the author is wrong because as per the manuscript the cold temperature (Tc) is the daily maximum temperature and the hot temperature is a function of net radiation, thus even if the fields are far apart (say 5 miles) still the hot and cold boundary condition would be identical.

Response: Again, this is not true on three accounts: (1) the Tc is not the air temperature but corrected by a factor, (2) 5 miles could bring differences in net radiation depending on hydro-climatic conditions, (3) actually the reviewer agrees with us that the hot and cold boundary conditions are identical for all lysimetes.

It is not clear how Dr. Paul arrived at the conclusion that our statement was wrong when we stated the same, except that we qualified by the statement of close proximity. This statement was useful to indicate the uniformity of hot and cold boundary condition is highly dependent on the hydro-climatic complexity. This is by the way one of the reasons that SSEBop can be applied for continental scale applications since each location has its own hot and cold boundary conditions.

Comment 23: Page 731 Line 24-26 "Because the modeling approach evaluates the Ts as a relative ET fraction between the hot/dry and cold/wet boundary values, the consistency of Eq. (5) across space and time is more important than getting the absolute magnitude right". This sentence does not make any sense mainly because the hot and the cold boundary condition for the dryland and irrigated fields are identical, implying that Ts needs to be accurately retrieved.

Response: Actually, it is the precision of LST and not the absolute accuracy that is important. The main reason is Tc is relative to the LST of the cold pixel which is obtained by relating LST of cold-pixel and maximum air temperature. Because Tc is relative to cold-pixel LST, Th is relative to Tc which is simply Tc +dT. The Ts (observed LST) is evaluated between relative Tc and relative Th.

Apparently, Dr. Paul has refused to accept the critical calibration/correction process that is followed to generate the Tc. Tc is not simply air temperature. Because of this he has refused to accept (1) the relative nature of Tc and Th and (2) their representation of instantaneous surface temperature values for the cold and hot pixels. It appears that he is confusing with the constant dT values for a given day and location. See more discussion on this under comment #9.

Therefore, Dr. Paul's assertion that the Ts should be highly accurate is not valid. It is more useful that it is consistent with the Tc and Th in precision.

Comment 24: Page 741 Line 2: "The SSEBop approach requires only satellite-based land surface temperature (Ts) along with a point or gridded reference ET (ETo) and air temperature (Ta) datasets" Did the author forget that they are using NDVI for emissivity calculation which comes from the red and NIR bands?

Response: This is a good point. A revision will be made to highlight that the importance of NDVI and emissivity when Ts is calculated by the user. But please note that in other sources such as MODIS-based Ts, the user directly uses available Ts without the need to go through the creation of Ts, i.e., Ts will be available for direct use.

Comment 25: Table 2: Reference ET value for 26 July 2007 in the irrigated field is 6.8 while 6.9 for dryland field. Why this difference?

Response; Thank you for caching this. This was a data entry error in the table where July 10 and July 26 were swapped positions.

Comment 26: The method proposed cannot qualify to be termed as 'surface energy balance' method because it is not solving the energy balance equation. The naming of the algorithm itself is controversial.

Response: It is true that it does not solve the full energy balance components, but like solving part of the water budget equation in watershed studies, we don't see anything wrong in using the name energy balance when we are actually using energy balance principles to solve A PART of the surface energy balance component. Besides that is why it is qualified with "simplified". We believe the reviewer has committed a straw-man fallacy by treating the effort as "silly" without reasonable supportive evidences, except casting a doubt about the naming of the model.

Comment 27: Listing all the errors in this manuscript is out of scope however the above mentioned points should be enough to gauge the value of this paper.

Response: We greatly appreciate Dr. Paul for taking his valuable time to provide these "short" comments. As we tried to point out in our responses, Dr. Paul had misunderstood the fundamental principles of the SSEBop approach. This led to several logical fallacies, hasty generalizations and ultimately wrong conclusions, claiming severe logical flaws and violations of physical principles.

While we respect Dr. Paul's legitimate and healthy criticisms of the manuscript, he mostly dwelled on minor issues that are largely nit-picky and inconsequential on the final results such as whether we can define the term dT this way or that. Particularly, it is unfortunate that he did not follow a useful line of questioning on whether the results made sense and if the evidences were plausible. To the contrary, he based his conclusions not from what was presented but from his own assumptions and new objectives such as the impact of order of aggregation on RMSE.

However, we will certainly improve the manuscript by articulating more clearly on misunderstandings that were caused by poor word choices or inadequate explanations and descriptions.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/11/C70/2014/hessd-11-C70-2014-supplement.pdf

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 723, 2014.