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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. Paul

george.paul@ag.tamu.edu

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"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. 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 con-

version 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.

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. 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.

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.

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?

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.

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?

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 credibility is questionable.

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.

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?

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.

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?

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

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?

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?

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).

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.

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). 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.

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.

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?

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?

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.

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.

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