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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.

Anonymous Referee #2

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Overall comments: The paper demonstrates the applicability of an operational approach (SSEBop) to multiple Landsat5 thermal infrared images for estimating daily evapotranspiration. The results are also evaluated against large scale lysimeter measurements established in four plots. I find many fundamental deficiencies of this approach. It is not understood that why a pixelwise estimation of net available energy (Rn-G) is not included. The estimation of Rn is also not described. Another serious limitation is the assumption of fixed value of the atmospheric resistance (rah) between the source height and air. This altogether ignores the spatial variability and temporal dynamics of meteorological and land surface conditions that have profound impacts

on rah. The value of the crop coefficient (k) appears like Priestley-Taylor parameter, but it is not explained in detail. A sensitivity analysis of the assumption of k = 1.25 is also missing in the manuscript. Which data the authors used for the atmospheric correction of thermal images, are also lacking in the manuscript. All these shortcomings must have been reflected in the results. Therefore, I cannot see the potential of this approach.

Specific comments: 1. Introduction: Citation of very old literatures. For example Tucker et al., 1979.

2. P.725, line 14. It appears Gowda et al (2008) work is the only work available for quantifying ET at various spatio-temporal scales. There are series of research by Norman et al. (1995, 2000 and so on), Anderson et al. (2007 onwards), Boegh et al. (2002 onwards), Bhattacharya et al. (2010) etc.

3. I do not see the introduction very compelling. There should be a concrete justification of using this approach.

4. p. 726, l. 11: replace 'exit' by 'exists'

5. p.728, L 25. What is the basis of choosing k equal to 1.25. There is no justification. Although a reference is given (Allen et al., 2011a), but this reference is not properly cited in the reference list.

6. p. 729, equation 3. To my knowledge, it should be Rn – G rather than Rn only.

7. Detail on Rn estimation is missing from the manuscript.

8. No attempt is shown to estimate the soil heat flux (G). Discounting G will lead to an overestimation of dT and underestimation of ETf and ET. If this is the case, then inclusion of G will entirely change the results.

9. Equation 4: How will you generate ET0 from remote sensing? Remember, you are labelling it as operational approach. So the estimation of all the variable or availability

of necessary inputs for estimating the process variables should be clearly stated.

10. How can you assume a single value of rah for the semi-arid regions? rah is influenced by the dynamics of the near surface boundary layer that included free convection and forced convection.

11. The authors did not conduct any sensitivity analysis on the assumption of fixed value of rah. Because the estimation of dT will be sensitive to this assumption.

12. Authors made an empirical correction of the errors fitting SSEBop ET to measured ET. This empirically increases SSEBop ET by 12 %. This is not a valid approach in my opinion.

13. The authors need to find out the probable reasons for the systematic error.

14. p. 736, L 1-2. It is said that the pixelwise estimates of the cold and warm boundary temperatures are similar for the four lysimeter fields. But among the four fields, two fields were irrigated and two were non-irrigated. The two field should differ in albedo and TS and resultant Rn. This shows the potential weakness of this approach. 15. No residual error analysis is carried out. Correlating the residual errors with the meteorological and land surface variables will be helpful to determine the potential sources of errors. Please see the paper of Mallick et al. (2014, Remote Sensing of Environment, ‘A surface temperature initiated closure for the surface energy balance fluxes’) on residual error analysis.

16. p. 735, Line9. “These dT values do not change from year to year for a given day of year”. This statement is not clear. Do you mean dT value is fixed for a certain day of year? This is also a serious limitation if that is true.

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