

Interactive comment on “Evaluation of an extreme-condition-inverse calibration remote sensing model for mapping energy balance fluxes in arid riparian areas” by S.-H. Hong et al.

Anonymous Referee #4

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The authors present work that compares SEBAL's estimation of energy budget components (mostly latent heat exchange) for 3 arid riparian areas, (Owens valley, CA, Middle Rio Grande Valley, NM, and San Padro, AZ) with ground based measurements. This work could be used to identify potential issues regarding the calibration and validation of remote sensing energy budget data specific to SEBAL against ground based measurements. However, as currently written, the manuscript appears unfocused due to either unnecessary detail and/or indirect writing. Due to the unfocused nature of the manuscript it is difficult for the reader to interpret the results or the validity of the study. Moreover, it is hard to discern what is the primary objective of the paper. For example,

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section 3.3 (Comparison of SEBAL flux predictions to ground measurements) seems to be (at least partly) the heart of the paper, but is under the methods section and is currently written as part methods and part discussion that appears to repeatedly fault ground based measurements for being inadequate to verify SEBAL results. However, from the title and introduction, I thought the purpose of the paper was to evaluate the performance of SEBAL in arid riparian areas, and not 'address the issues of comparing satellite based energy budget data to ground based data'. Furthermore, at L4-5 on page 13496 the aim is restated to 'evaluated the challenges of SEBAL flux predictions in arid riparian areas using a validation approach', which is more in line with sections with section 3.3's discussion. However, the approach discussed in section 3.3 leads me to believe that ground based measurements may not be an appropriate method to validate SEBAL and that comparisons to other satellite based methods are warranted given the scale differences between ground based measurements and satellite observations. However, the decision of whether or not to include further comparisons hinges on what the specific purpose of the paper is, which needs to be better defined by the authors.

Major Comments: Page 13481 L13-L14: It is actually not clear to me that the work presented here provides evidences that SEBAL yields reliable estimates for actual evapotranspiration rates in riparian areas of the southwester United States. Primarily, because of the issues presented in section 3.3 as well as the results section that painstakingly point out the issues with ground based measurements, which are the only set of validation data used in this study.

Page 13487 L4-L6: Why is it safe to assume that soil moisture is constant? Especially for arid environments where ET usually is a large part of the water budget? What are the implications of this assumption?

Page 13487 L17-L18: Support for the assumption that $G_{24} = 0$ should also be stated here rather than later at Page L13492, L26-L27. Regardless this to me seems to be a rather large assumption that has consequences as the land surface and soil column

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in these environments will experiences seasonal (larger then daily) scale warming and cooling.

Page 13491 L3-L8: Why do you exclude data were the sum of the H an LE is 65% less then or 110% greater then of the available energy? This criteria seems to eliminate much of the available data. Is this an indication of poor observational conditions, such as days that are not clear or have variable weather?

Section 3.4 Footprint model: There are important concepts that are partly presented here regarding the problems of validating SEBAL using ground based measurements, which I believe contributes to a lot of the issues of calibrating and validating SEBAL. Mostly reconciling scale issues between satellite observations and point scale(ish) measurements. These issues are cryptically mentioned in sections 3.3.2 and 3.3.3. For example the difference between the heat plate scale of 0.001 m² and 900m² land-sate pixel. Unfortunately, the scale of measurements and the scale of the SEBAL pixel is never clearly or coherently presented, rather some information about the scale is peppered throughout the paper.

Minor Comments: Page 13483 L13-L22: These two sentences have unnecessary detail, that can be summed up as field measurements are slow and costly, in contrast satellite measurements are fast. Please be more direct.

Page 13483 L20: Change ‘...86000 ha of the office...’ to “...86000 ha from the office...”

Page 13483 L22: The phrase ‘expert months’ is not clear.

Page 13484 L6-L8: “Another difference with previous studies is our focus on all components of the energy balance during the instant of satellite overpass ...” Is this the only difference? Did the other validation studies not focus on arid riparian areas? Also, did those validation studies have the same problems with ground based measurements discussed in section 3.3?

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Page 13485 Equation 2 and 3: Is H instantaneous or daily? It appears the notation is not consistent.

Page 13487 L17: Why is Cef set to 1.0, which would then have no effect on equations 5 and 6?

Page 13493 L19 – Page 13494 L10: This paragraph is an example of indirect writing. I assume the point of the paragraph is the last sentence, “Therefore, in this study rather than trying . . .” In scientific writing, the point should be stated up front and supporting details follow the main point.

Page 13500 L19: ‘Incoming short and longwave radiation’ and for that matter outgoing short and longwave radiation. These terms can be separated from R_n in your equations and in many energy balance equations can be calculated/measured separately. Perhaps the terms should be presented as separate components of the energy balance equation (equation 1).

Page 13503 L22: The phrase ‘traditional SEBAL’ is awkward.

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