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# ***Interactive comment on “Remote sensing techniques for predicting evapotranspiration from mixed vegetated surfaces” by H. Nouri et al.***

**Anonymous Referee #2**

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The paper by Nouri et al., entitled “Remote sensing techniques for predicting evapotranspiration from mixed vegetated surfaces,” provides an overview on past technologies for remote sensing of evapotranspiration. The authors qualitatively present a number of studies, without going into quantitative depth.

Overall, this paper only provides an overview of some techniques employed in the past to remotely sense ET. The motivation for the review is not entirely clear, as at some points urban conditions are brought forward, while the title promises vegetated surfaces. Towards the end, the authors favour high-resolution satellite products that are very expensive and have a very low temporal resolution, and therefore are not appropriate for large scale applications.

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On the technical side, this paper lacks the in depth revision of techniques, as it was presented in review papers such as Kalma et al. (2008). There is no quantitative comparison of the quality of the products discussed nor are any applications shown or the potential errors quantified, when applying remote sensing data to the SVAT models. In the light of this and my detailed comments below, I do not recommend this paper for further publication.

Major comments:

Introduction : the first paragraph of the introduction is void of references and statements are not backed up with adequate sources. This also occurs throughout the text, making it difficult to justify certain statements. In fact, the first citation is a self-citation of something that has already been known for quite some time (p. 3899, l. 1-3). There are no references on recent remote sensing products, such as the one developed by Eric Wood's group in Princeton. If this is a review paper on algorithm development, then it should include a discussion on recent products made available to the larger community.

I agree that traditional techniques, such as flux towers only provide a point measurement (or better a footprint), but the authors should look into the use of scintillometry to extend this to several kilometres of scale (p. 3900, l. 1-3).

Lack of references p. 3900, lines 13-16 and need for more recent references later in the same paragraph.

What is the purpose of referring to an urban RS paper (p. 3900, l. 22-25). I am not saying that it is not an adequate reference, but it needs to be better justified (also when referring to it later in the text (p. 3905, l. 15-17; p. 3906, l. 10-12).

How can we actually make sure that our calibrations are accurate (p. 3902, l. 14-16)? And what are the implications, if we cannot determine them directly and accurately? In terms of error characterisations (p. 3902, l. 27-29), I suggest to have a quantitative

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discussion here, rather than a simple statement.

Given that most ET products rely on a dry reference within the scene, what are the implications, if this is not the case, due to humidity, cloud cover, or vegetation (p. 3904, l.2)?

Vegetation height and LAI etc. are mentioned as input parameters (p. 3904, l. 7-8, p. 3905, l. 3-4). How would we accurately acquire those, if not through in-situ point measurements?

P. 3904, l. 8-15: this is a very vague description/discussion. It mentions SEBS, but goes directly to SSEB, without really discussing SEBS itself. What were the results of senay et al? At this point, I need to ask: what is the purpose of this review in general? So, far it's not more informative than Kalma et al., and much less detailed.

If Allen et al. (2005) stress the point that the findings are not easily transferable, what does it mean for other applications (p. 3905, ;l. 24-26)? What can we do about it to make these findings transferbale?

P. 3906, first paragraph: the authors should look into more recent literature, such as Pipunic et al.

What is the purpose of referring to the Mauser and Schadlich (1998) paper? PROMET may be a SVAT model, but what is the purpose of explicitly mentioning it here in a single sentence.

Do we really need regular airborne resolution data for our applications (p. 3907, l. 13). I feel that Landsat and the future Sentinel satellites will provide us with free access to sufficiently high-resolution data in the future.

What are all those references referring to (p. 3907, l. 14-17)? All the authors of this paper mention beforehand is riparian vegetation. This long list of papers deserves a much more detailed discussion.

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Freely available, optical RS data are generally corrected for atmospheric and solar effects (p. 3907, l. 19) and it is quite well known that optical satellites cannot provide any information on the surface conditions during cloudy days, this is stating the obvious (p. 3907, l. 20-21).

What is the important role of climate data in SEBAL and how does it affect the model results (p. 3907, l. 23).

P. 3908, l. 1-4. I do not believe that such a sentence is appropriate for a research publication. If the authors “thoroughly agreed with Allen’s viewpoint”, then it would be worthwhile discussing this in detail and justifying it.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 3897, 2013.

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