

Interactive comment on “A new method to calibrate aerodynamic roughness over the Tibetan Plateau using Ensemble Kalman Filter” by J. H. Lee et al.

Anonymous Referee #2

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General comment: First, the paper needs to be reviewed at all. It is badly organized, not clear and difficult to read and follow (just one example: in the last paragraph before the conclusion section the results of modeling efforts at the Landriano site are reported. But Landriano is not in the Tibetan Plateau and the site was never introduced in the method section or in the introduction. The methods are not fully described. I have numerous comments, but for brevity I'll report below a list of main comments. Second, the objective may be interesting for publication but it needs a deeply study revision and analysis. However, main concern is the need to show how the proposed approach is better than other existing approaches in the literature, and what is new compared to

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other approaches.

Comments:

1) Introduction section: it needs a deeply revision since the main objective of the paper, data assimilation method for aerodynamic roughness height (Zom) estimation is not introduced actually. My guess is that the new contribution is the use of data assimilation for a sort of dynamic calibration of Zom. I can't see any reference on attempts made on this topic, not for Zom or for other land surface model parameters (e.g., Moradkhani et al., 2005, Montaldo et al., 2007). Relative to this last reference I see that it is in the reference list but it is not cited. The problem of the citations and references will occur several time in the reading, such as I'll remark below in some comments. However, regarding other attempts made on this topic there is the need to compare the proposed approach with the previous, what are the differences and what are the new contributions.

2) pag. 5197, row 27: "Consequently, errors in these parameters can be propagated through models and become a major error source in the output of those models.". need a reference, otherwise I'm not convinced that all the models (AROME, Wasp, NOAH, CLM) are so much affected by errors of Zom.

3) pag. 5198, row, 4: again references for that equation, or explanation on how it is derived.

4) pag. 5198, row 10. "Estimation of aerodynamic roughness is usually performed under neutral (i.e. or near-neutral condition": again, reference.

5) pag. 5198, row 19-20: "...several previous studies employed..." but you are not citing any previous study. Again, reference.

6) pag. 5199, row 9. Figure 1 doesn't seem necessary. It comes from an old paper (1983), and compares a literature value (and it is not explained how it is derived), a SEBS value (and it is not explained what it does mean) and an estimate from MODIS.

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For instance, field estimations (from eddy covariance data – wind profile method) are not reported. However, just put a reference.

7) pag. 5199, row 12. In this introduction after you introduces the figure 1, suddenly a description of values of MODIS NDVI in BJ station starts. You never introduced MODIS and, especially, what is BJ station? Furthermore, the values reported in the text are not in Figure 1. It is very difficult to follow this section.

8) pag. 5199, row 15. Then SEBS is cited. But what is SEBS? Then there are NDVI1 and NDVI2. But what are they?

9) pag. 5200. Last paragraph of the introduction. Suddenly data assimilation is introduced. EnKF is not defined. EnKF is not introduced. Nothing on Kalman filters or other data assimilation methods. No comparisons and references with existing methods for parameter estimation (calibration) with data assimilation.

10) pag. 5200. In both the last paragraph and at the start (row 13) of the “Method section” there is written that EnKF (never defined) was used as Kalman filter. But after several pages (pag. 5205, section 2.3) I discover that EnKF was not used but the DEnKF was used. But then after this section the paper recall the EnKF and never the DEnKF. What is the Kalman filter used?

11) pag. 5200, rows 14-15. For a correct evaluation of the proposed approach, the Kalman filter needs to account for errors of the model inputs (e.g., precipitation, wind velocity, other meteorological inputs) and not of Zom only. Zom is just a parameter. Furthermore for a deep evaluation of the proposed approach errors other parameter errors should be considered. Perhaps the soil parameters (e.g., saturated hydraulic conductivity) can affect much more the latent heat flux. A sensitivity analysis may be helpful.

12) pag. 5200. Section 2.1 Field measurement. The field site is not described at all. No vegetation type, no soil type, instruments, etc. The paper reports that at the Naqu

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site in 2006 a Bowen ration station was working. However, in the only reference (van der Velde et al., HESS 2009) the bowen ratio station worked for a short period (3 -10 September 2005), and for another short period (16-26 April 2005) where also an eddy correlation did work. Hence, no 2006. How can it be possible? In the figures 3, 5, 6 and 7 there are long time series of almost 120 days. Hence, they cannot come from van der Velde et al. (2009). It seems that the reference is not correct. what is the year? Hence, there is the need of explaining where the data come from, what are the instruments, why in the van der Velde et al. (2009) paper that long time series was not used.

13) pag. 5201, row 16. This sentence needs to be demonstrated or references need to be added.

14) pag. 5202, rows 10. The admission that at Naqu sites vapour pressure gradient ($vpg=e_1-e_2$) is small is a main concern on the accuracy of Bowen ratio (β) data on this field. Indeed β is inversely proportional to vpg , and low values of vpg produces high values (toward infinite) of β although the temperature gradient may be accurate. Since from van der Velde et al. (2009) eddy correlation data are available in 2005, why are you not using those data (which should be more correct; indeed, in van der Velde et al. (2009) a check of the bowen ratio quality data was made using the eddy correlation data).

15) field measurement: since you have eddy covariance data, you can make of an estimate of Z_{om} through the wind profile method based on eddy covariance data. Furthermore, since the paper is focused on Z_{om} , why not an estimate of this parameter from the field?

16) section 2.2.1: this section can be reduced. The equations come from a well referred model. Perhaps put them on the appendix.

17) section 2.2.2: again, this section can be reduced. The equations come from a well referred model. Perhaps put them on the appendix.

18) row 7-8, pag. 5204: reference of Su et al., 2002 is not in the reference list.

19) pag. 5205, rows 9-14: in this first paragraph of the section the deterministic Ensemble Kalman filter (DEnKF) is introduced. Again (see comment 10), before they mention the EnKF only. This needs to be clarified. Second, this paragraph needs to be in the introduction section, and needs to deeply explain: why the choose of using this kalman filter, attempts made by other authors, and what is new. Furthermore, the reference Sakov et al., 2008 is not in the reference list. The reference is Sakov and Oke, 2008. Also Reichle et al. (2008) is not in the reference list. Again problems with references.

20) pag. 5205, row 5. Suddenly the equation (3-1) is reported. What is that? It is not introduced. I supposed that it is equation (6) of Sakov and Oke, 2008 (why not just put the reference?) but, correctly, in that equation X_i was used instead of X (as made in (3-1)) because it is for each ensemble member. Indeed, in (3-3) you use X_i correctly.

21) pag. 5205, equation (3-2): what is “[$x_a \dots x_a$]”? never defined.

22) pag. 5206, rows 16-17: how was generated the ensemble? Monte Carlo simulation? What are you changing for model inputs? NDVI is a parameter, you need to change model inputs (see also comment 11).

23) pag. 5208, rows 1-4. I don't understand this paragraph. And NRRH and NRRLE are not defined at all.

24) pag. 5207, row 7: again is EnKF or DEnKF?

25) pag. 5207, rows 16-20. I can't follow the text. First, is SEBS calibrated? You need to show Zom effects on model outputs when other model parameters are or are not calibrated.

26) pag. 5207, rows 21-24. Again, this sentence needs to be demonstrated. Wou need to show results (graphs for instance) and explain better your results. In this sense, did you make any sensitivity analysis of the model? If yes, what type of analysis? Global

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multivariate? Again, first of all you need to show the calibration and validation of the model.

27) pag. 5208, rows 2-4. Again, suddenly two equations are reported. What are them? They need to be introduced and explained. Equation number is also missing.

28) pag. 5208, rows 8-12. Fig. 3 shows first assimilation results. Again, the problem of EnKF or DEnKF (it will occur many other time. . .). However, you need to show the ensemble open loop run without assimilation for comparison. You need to show the spread of the ensemble behavior and compare it with the ensemble open loop run. At the moment it is not clear if the model is calibrated or not (are the other parameters calibrated or are they “guess parameters”?), if the assimilation is working and how it is working dynamically. Furthermore, at the moment it is not clear what data are these. Time series is of what year? Again in van der Velde et al. (2009) paper this long time series doesn't exist.

29) pag. 5208, row 16. Are you considering errors in NDVI (see end of page 5206) or aerodynamic roughness height, such as written here? There is confusion.

30) pag. 5208, row 17. “. . . This estimate was considered. . .” what is this estimate?

31) pag. 5208, row 24. Fig. 4 is cited but it is not explained and clear.

32) pag. 5209, row 12. “. . .original SEBS. . .” again, what is the original SEBS? Did you calibrate it? Dis you use literature values for the parameters?

33) pag. 5209. “Julian day”. Such as well known these are not Julian days. They are “days of the year”. The year is not reported.

34) pag. 5209, rows 19-22. I don't understand the connection between “soil moisture and precipitation were reported very high during this period” and “BREB sensible/latent heat estimates were considered reliable by water balance”. Please explain it. I was thinking the opposite.

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35) pag. 5210, equation (4). Another equation without introduction. Furthermore I can't understand why this equation is here and what is the usefulness of this equation.

36) pag. 5210, row 12. From Figure 6 I can't see any bowen ration time series. It could be interesting to show.

37) pag. 5210, row 14: ("data not shown"). Why are you not showing? You have space and there is the need of more figures of the results.

38) pag. 5210, rows 12-26. This discussion is not really interesting and I can't see anything really new. It may be reduced.

39) pag. 5210, last rows: did you make any energy balance check? Did you check the values of the available energy?

40) pag. 5211. At the end of the result section, suddenly results at Landriano site (in Italy) are reported. First in the title of the paper there is written "... over the Tibetan Plateau...". Hence, the title of the paper must be changed. Second this site was never introduced. No site description, no instrument descriptions. In the method section nothing on this site. Climate and environmental conditions are different at all. And you are not showing any results. No figures, no graphs.

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