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

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We appreciate your interest in this paper, and your introducing interesting papers. We looked over Gupta (1999) and Xia(2002). It is considered that this study is a lot different from those previous studies, in terms of whether errors of the heat flux itself to be used in cost function minimization were treated or not. One of the main aspects that this study suggested is that heat flux measurement error itself can be propagated and contaminate estimation when inversely inferring parameter. This is not trivial in regional scale or remote sensing study. Depending on whether data assimilation is

used or not, they provide a different parameter value. Next, this study does not suggest Multicriterion. Especially, in a case of Naqu site, single criterion - sensible heat measurement without requiring other state variables - was used to estimate aerodynamic roughness. It is considered much more simple, and useful, leaving out all the complicated calculation and computation time. Please note that if more number of measurement or criterion is added, uncertainty and error propagated through models increase. This is not multicriteria or multi objective method that confers a optimal range -not a unique solution - and trade-off conflicts among each criterion. If looking into 'gaussian error propagation' in Results 3.1, this operational frame did not consider several input parameter errors equal. Decisively, Gupta (1999) and Xia(2002) did not show aerodynamic roughness height, compared with other vegetation index or wind profile methods. There is no way to assess or validate parameter estimated in those studies. Thus, it is difficut to compare them with this study, since approach of this study is to improve heat flux by adjusting aerodynamic roughness. Aerodynamic roughness height, it is believed, distinct from other paramaters. There are several issues with aerodynamic roughness parameterization. Actually, there are several previous studies who attempted to minimize a cost function to estimate aerodynamic roughness height specifically. However, this approach is affected by several other involved input parameter or instrument errors in Monin-Obukhov Similarity (MOS) equations, while this study requires only one criterion, sensible heat whose error was considered to be treated by data assimilation. Each number corresponding to your comment was listed below.

1)Every parameter and instrument contains error. Among those, this study focused on common but specific input paramater: aerodynamic roughness height. Relatively speaking, other input parameters such as land surface temperature or soil moisture are directly measurable. It is very clear how to measure them. However, aerodynamic roughness height is somehow not straightforward. Locally, EC methods are often used. However, in several cases, this does not satisfy assumptions - this is already discussed in the context. Even regionally, and globally, VI method has a limitation in application. So, this study focussed on adjustment of aerodynamic roughness height. Regarding

soil heat flux, please refer to van der Velde (2009). Additionally, as you and this study already mentioned, BREB method assumes enery closure. So it was emphasized that this limitation of BREB was treated by data assimilation using SEBS estimates that does not assume energy closure, and that unreasonable parameter values were rejected by frequency analysis. It was anticipated that SEBS compensated for BREB or vice versa. Although BREB data were selected by parsimoneous data filtering as described in methodology, this aspect will be included in discussion part.

2) We were awared of and discussed this aspect. When establishing data assimilation frame, zoh was designed to be purburbed by zom. As you mentioned, zoh is very important in SEBS physical structure. However, preliminary examination(not only Yang but several other formulations were also tested) found that zoh did not change heat flux results very much. Rather, contribution of stability or obukovlength was considered more influential.

3)You mentioned that this paper emphasizes that sensible heat flux is always more than latent heat flux and is a dominant energy source: Let me revise this expression more clearly in final revision version. Usually in arid region, sensible heat is, as readily anticipated, higher than latent heat probably "always". Main energy source in Tibetan plateau is also known as sensible heat, according to several previous studies. However, interestingly, in semi-arid region, during Monsoon period, this phenomenon gets reversed. It was demonstrated that latent heat is higher than sensible heat during Monsoon precipitation. It was curious if sensible heat still remains 'accumulative' dominant energy source even after experiencing summer Monsoon or not. This was intriquing because soil moisture in deep soil layer is very low (around 0.07 m3/m3) even during Monsoon. Without calibration that this study suggested, this discrepancy between latent and sensible heat was shown very large. Discrepancy between latent and sensible heat, if using calibration, is 17 W/m2,approximately, during Monsoon. You also suspected that latent heat flux is comparable to sensible heat during monsoon - this is what this study tried to show in figure 6.

Yes, there are EC data in other years, as previous study (van der velde, 2009)validated.

4)To avoild some confusion, all the K characters were explained below the equation. von Karman constant K will be changed to italic small character k. K in (3-1) is Kalman gain. KB-1, excess resistance will be changed to kB-1. This paper used Zoh and Zom, according to previous study, Su(2002). Actually, this indication seems little bit different in each paper. With regard to stability term, you may also refer to the same reference. It is all psi_h.

5)One of the reasons that we often employ a logarithmic scale when indicating the aerodynamic roughness height could be because it presents exponential increase or decrease in wind profile, handling some spike in a very large range. However, in expression of the 'frequency' in Figure 4, it is comparing the frequency itself - not aerodynamic roughness- in each manageably small segment of aerodynamic roughness. Obviously, it could cause some bias in visual. Second, regarding the only outlier, theoretically, it is very possible that z0m near Julian day 164 is bigger than it near Julian day 169. By definition, aerodynamic roughness is determined by momentum activity. For example, if comparing momentum drag force with geometric vegetation height, those two move independently. Drag force stays more or less the same while vegetation height continuously increases over time. However, essentially, it is a momentum highly fluctuating by wind and affected by atmospheric stability. If estimating aerodynamic height by traditional wind profile methods, it is more fluctuating than this trend line. A techincal explanation for this rising on Julian day 164 is that sensible heat EnKF final analysis is likely to be overestimated, considering SEBS sensible heat estimates using EnKF calibration. In detail, SEBS estimation using EnKF calibration (155 W/m2 on Julian day 164; 160 W/m2 on Julian day 169) is lower than EnKF final analysis (200 W/m2 on Julian day 164; 135 W/m2 on Julian day 169). Therefore, this may imply that a method combining EnKF and SEBS is physically plausible. Although BREB was used as a truefield, it may not neccessarily mean that it should be absolutely true because this study only applied data assimilation into the estimation of intermediate parameter,

which finally adjusts and determines heat flux through SEBS physics.