We appreciate your comments on this manuscript.

Although aerodynamic roughness height is difficult to determine, no much attention has been paid to it at local as well as large scale. Most land surface models employ established approach in determining the aerodynamic roughness, whose limitation and issues were discussed in this paper.

This manuscript was organized in a very systematic way: 1) introduction: brief introduction over Tibetan plateau was introduced in terms of land surface and convective condition. The definition of parameter of interest was kindly reminded for readers. After the various previous approaches for the determination of this parameter was summarized and introduced, issues and limitations for each previous approach were interestingly discussed. These arguments and points to be made in this paper were considered as the research gap explaining the need and usefulness of this paper. Significance and difference (or uniqueness) of this study from other previous studies were also addressed in introduction as well as AC to the 1st reviewer.; 2) methods: BREB method was first demonstrated since it was used as a true field in Ensemble Kalman Filter. SEBS (the one that you asked in comment # 8) model was also briefly described in terms of parameter and heat flux estimation to explain how ensemble member was perturbed and propagated through model. Finally, data assimilation theory to reconcile those two methods was introduced; 3) results and validation: Landriano station data with different climate or convective condition from the Tibetan plateau were demonstrated for a validation purposes only to offer the versatility in this approach. All the model settings and approaches are exactly same with Naqu site so that it was considered that there is no need to repeat it over and over again. This showed that this approach can replace eddy covariance methods even under wet condition like Landriano station, although this study focused on the very dry condition such as Naqu site that eddy covariance data are not available. If this study can accommodate a range of climate with a slight variation, it was believed that readers can flexibly establish their own site by getting an idea from these examples; 4) conclusion: overall findings and approaches were reviewed and summarized again.

In comparison with other previous studies, what the new aspect of this study is was redundantly described in the original manuscript and in AC #1 already : 1) few previous study suggested operational frame to apply the data assimilation into aerodynamic roughness height determination; 2) the number of influential parameter required to minimize a cost function is less than other previous studies (only one parameter: sensible heat in naqu site example); 3) this approach is less affected by the saturation impact of remotely sensed vegetation index; 4) unlike literature values which stays constant during and after Monsoon dynamics, this approach can describe time-variant parameters affected by convective behaviors; 5) this can replace eddy covariance methods available only at local scale. Following numbers reply to your comments, respectively.

1)

RC) The main objective of the paper, data assimilation method for aerodynamic roughness height (Zom) estimation is not introduced actually.

AC) As written in the introduction part and abstract, few previous study has applied data assimilation especially for the determination of aerodynamic roughness height. It strengthens a value/new aspect of this paper. If you would like to introduce excellent previous study that significantly dealt with it, that reference will be very much added in a final revision. Otherwise, data assimilation method for aerodynamic roughness height (Zom) estimation is being introduced in current paper as a novelty. Review for previous data assimilation study applied into other land surface parameters was not repeated, since it was published in several other studies already.

RC) Relative to this last reference I see that it is in the reference list but it is not cited:
AC) Moradkhani et al., (2005) was cited on line 17 on page 5206. Montaldo et al., (2007) estimated root zone soil moisture –NOT aerodynamic roughness height- using different formula from this study, although it confers a very interesting frame for application of data assimilation into hydrology. Thus, it was not directly cited in the context but indirectly referred in the overall frame.

RC) There is the need to compare the proposed approach with the previous, what are the differences and what are the new contributions.
AC) Please refer to figure 1 and Landriano data (comparing the approach of this study with eddy covariance, and VI using formulation) in result section to show the agreement from other methods. New contribution and findings were discussed - in abstract, result and conclusion part -.

"The advantage of this approach over other methodologies is that aerodynamic roughness height estimated in this way is useful even when eddy covariance data are absent and is time-variant over vegetation growth, as well as is not affected by saturation problem of remotely sensed vegetation index." – Abstract.

According to review paper (Ma *et al.*, 2009), it was said that a dominant energy source in Tibetan plateau is mostly a sensible heat. Current study demonstrated that it is not the case during Monsoon period. Sensible heat is just comparable or less than latent heat during this period. Please refer to comments replying to 1st reviewer.

2) Please, more accurately read the line 23-25 on page 5197. It was said that aerodynamic

roughness is important in those model physics, not saying that input errors were propagated via NOAH, WASP, and CLM. Nonetheless, reference can be provided (Chen et al., 2010, Zheng et al.,2007). For AROME, please refer to Figure 1 in this study.

Chen, Yingying, Kun Yang, Degang Zhou, Jun Qin, Xiaofeng Guo, (2010). Improving the Noah Land Surface Model in Arid Regions with an Appropriate Parameterization of the Thermal Roughness Length. J. Hydrometeor, 11, 995–1006. doi: http://dx.doi.org/10.1175/2010JHM1185.1

Zeng, Xubin, Aihui Wang, 2007: Consistent Parameterization of Roughness Length and Displacement Height for Sparse and Dense Canopies in Land Models. *J. Hydrometeor*, **8**, 730–737. doi: http://dx.doi.org/10.1175/JHM607.1

2)It is already cited in there - Ma et al., (2008), where it is informed that Chen et al., (1993) derived it. This study also reached to the same formulation as below:

$Cd= \tau/(\rho U^2) = ((U^*)/U)^2$

where, ρ = density of air, U= mean wind velocity at reference height, U^*= friction velocity, τ =shear stress, Cd=drag force. Since $U/(U^*) = 1/K \ln(Z/Zo)$, Cd can be rewritten as followed:

$Cd=k^2 [\ln(z/z0)]^{(-2)}$
$K/\sqrt{Cd}=\ln(z/zo)$
$Zo=Z \exp(-k/\sqrt{Cd})$

This is the same formula on page 5198.

4) it is already cited in there: Yang et al., (2003):

"Using the wind profile under neutral conditions to determine z_{m0} is a common approach (e.g., Kohsiek et al., 1993; Grimmond et al., 1998; Zhou et al., 2000), but it is sensitive to measurement errors." On page 250 of Yang et al. (2003).

Please note that, as described in line 20 on page 5208, during a highly convective period, only unstable condition was used.

5) Olioso (2002) and Su (2001) will be added. It is a general sentence, which is further split into several supporting sentences. Those are further described by each reference, respectively.

6) RC) However, just put a reference.
AC) Data were made by this study so that there is no need for putting a reference. The SEBS formulation to derive those data was explained in method section. I don't understand what old paper (1983) means. It is newly derived data for a case of grassland in 2006 (Tibetan plateau). The implication for this comparison is obvious, since grassland has a different LAI sensitivity and range from other vegetation types when aerodynamic roughness is formulated with vegetation index. For example, the same formula may work fine for crop (low vegetation) or other types of vegetation, while it may not work for a specific vegetation type at the grassland level examined in this study. Accordingly, it is suggestive in a perspective of vegetation type sensitivity or MODIS products.

7) In introduction part on page 5197 line 6-8, BJ station is already informed in the very beginning of manuscript.

"This study is based upon turbulent meteorological data measured at the BJ station located in the Naqu site, one of Tibetan Observation and Research Platform (TORP) under the frame of GEWEX"

As explained in method page 5203, NDVI and LAI are used for determination of aerodynamic roughness. As discussed in AC # 6 of this posting, point of figure 1 is to show bias and limitation of currently used aerodynamic roughness height estimation in case of a grassland under a given climate condition. MODIS is just one common way used to acquire the vegetation index. Issues associated with MODIS bias were cited using previous reference (Yang et al., 2006). For those readers who have a strong intriguing in MODIS beyond a context of this study, please refer to previous study as followed.

Warren B. Cohen, Thomas K. Maiersperger, Zhiqiang Yang, Stith T. Gower,
David P. Turner, William D. Ritts, Mercedes Berterretche, Steven W. Running. (2003). Comparisons of land cover and LAI estimates derived from ETM+ and MODIS for four sites in North America: a quality assessment of 2000/2001 provisional MODIS products. Remote Sensing of Environment 88: 233–255.

RC) Furthermore, the values reported in the text are not in Figure 1. It is very difficult to follow this section:
AC) Of course, MODIS NDVI and LAI are not shown in figure 1, because they are just an input of aerodynamic roughness height formulation. Those numbers are just read from fully processed satellite end products.

8) RC) Then SEBS is cited. But what is SEBS?
AC) as written in methods on page 5202, SEBS is the Surface Energy Balance System used in this

entire study. $NDVI_1$ and $NDIV_2$ are adjacent pixel numbers randomly selected for relative comparison.

SEBS abbreviation was noted in abstract in the very beginning.

9) RC) EnKF is not defined. EnKF is not introduced.
AC) Detail introduction and description for EnKF is given in method page 5205. Again, to our knowledge, no previous review is available for ensemble kalman filter determining aerodynamic roughness height. Rather, this study introduced other previous 'cost function minimization' approach applied to aerodynamic roughness height. Of course, it is also very possible to briefly introduce some general overview for ensemble kalman filter itself. However, it would not include aerodynamic roughness height.

RC) No comparisons and references with existing methods for parameter estimation (calibration) with data assimilation.
AC) comparisons with existing methods are newly given in this study result part. Existing method for parameter estimation using data assimilation was introduced as Moradkhani et al., (2005) in line 17 on page 5206. (No further existing methods for aerodynamic roughness height was found). This reference can be additionally explained in introduction of final version if requested. However, this is not estimating aerodynamic roughness height. In addition, this study perturbed several other input parameters together, which was only indirectly validated with the third observable estimate of stream flow forecasting because a state variable updated by input parameter was considered unobservable. It is different from this study in that current study estimated unobservable and indirectly estimated aerodynamic roughness input parameters, which was further validated with observable heat flux. Moradkhani et al (2005) used a forward mode, while this study used an inverse tracking somehow similar to inverse mode and Look Up Table approach of SAR soil moisture retrieval.

10) Deterministic Kalman filter is one stream of Ensemble Kalman Filter. I believe this would not make much confusion for future readers.

11)

Soil type or porosity is one of the factors influencing on latent heat. However, unlike TOPLATS or SVAT (considering soil water flow or Richard's equation), SEBS is a surface energy balance system, not reading soil parameters or profile such as saturated hydraulic conductivity (Su et al., 2002; Li et al., 2010). Saturated hydraulic conductivity does affect the evolution of water flow – as discharge or infiltration – that exploits latent heat which is as a major defined by aerodynamic roughness and resistance as well as vegetation height etc. Latent heat and saturated hydraulic conductivity are all inputs in water balance. Instead, LAI or vegetation height or wind profiles are major inputs for latent heat flux. Detail formulation for SEBS is already described in methods.

Furthermore, if looking over an operational frame in Figure 2, and data acquisition methods, other meteorological parameters were assumed to be accurate when compared with the uncertainty of aerodynamic roughness height, which was described in introduction. Other meteorological parameters such as wind velocity, pressure, humidity or temperature are very straightforward in measurement. These are all directly measured from a local station that offers highly reliable estimates. It is reasonable to assume these inputs are certain, if reminding that some other parameters such as precipitation are a main source of uncertainty in a remotely sensed large scale study where local meteorological station data are not available. Please note that their validation and calibration study of satellite are all based upon 'field measured' local station data as a true field. On the other hand, aerodynamic roughness height is only indirectly estimated with a large scattering. It is unobservable and not straightforward especially in a large scale because remote sensor can't read a vertical characteristic of each object.

In closing, a variance for the probable latent heat errors stemming from unknown several other input parameters was considered and defined in data assimilation regime on page 5206.

RC) Z0m is just a parameter.
AC) It is not just a parameter, but a major input as written in page 5197, and demonstrated/discussed on page 5207, and shown in Figure 6, on page 5223. It is a major objective to demonstrate. We have spent the entire text in introduction and result to show z0m is not just a parameter.

12)

RC) The field site is not described at all. No vegetation type, no soil type, instruments, etc.
AC) It is described in introduction. It was informed that vegetation type is grassland (figure 1), and soil type and instrument were all cited by previous study (van der Velde et al., 2009, 2010). Please note that this is not the first paper to introduce this area.

RC) The paper reports that at the Naqu site in 2006 a Bowen ration station was working.
AC) It is difficult to understand what Bowen ration station implies exactly. If you mean the BJ station in Naqu site over Tibetan plateau, reference used in here is a correct one. Whether 2006 or 2005 is not the point. This reference demonstrated that Bowen ratio method is agreeable with eddy covariance method in a given range (50 up to 300~450 W/m2) and climate condition (in a given temperature and vapour pressure). How much range and climate it can accommodate is more important than a length of period. It is not like that BREB error is propagated by time

sequence or amplified by time accumulation. A source of BREBE error is based upon temperature gradient and vapor pressure. Those are controlled and filtered out in method 2.1. on page 5202. Additionally, please, note that eddy covariance is also not absolute truth. Those error ranges were specified in conclusion part using a specified reference. In fact, if you accurately read through this manuscript, there is no need to heavily rely on BREB exclusively, because a parameter estimated has gone through SEBS physics again after determining aerodynamic roughness as an input. Basic idea is that BREB is compensated by SEBS. This may be considered as one of the differences from other data assimilation studies that estimate a final analysis for output of land surface model.

RC) 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.

AC) that is a correct reference. You can also see van der Velde et al. (2010) and Dr. Zeyong Hu in acknowledgement.

13) Reference is this study. It was demonstrated by filtering. Please see page 5201-5202. It is a common basis that data assimilation goes towards a true field. It is based upon informal communication with several anonymous professors, scholars and algorithm developers.

14) It is already discussed and explained. Please see line 10 page 5207. That is why this study selected sensible heat only.

RC) Since from van der Velde et al. (2009) eddy correlation data are available in 2005, why are you not using those data
AC) please remind the objective of this study. This study contributes a new method to those who have no eddy covariance data available in their local or large scale. This is repeatedly noted in the abstract, introduction, and conclusion. Eddy covariance is only locally available, and expensive, but still contains a degree of error.

15) I don't understand this. Eddy covariance data were not available in 2006, as written on page 5200. The limitation of wind profile method is already discussed in introduction, along with further reference.

16) Only relevant part (how aerodynamic roughness is determined and it is related with heat flux) was extracted out of a lengthy model description in a complete set. Moving it to appendix is possible. However, if considering your comments number 2, 7, 8, and 11, it is believed that a description should be provided in there not to make readers any confusion but to provide sufficient information – How error of input parameter was propagated. Instead, it will be

considered that data assimilation theory is moved to appendix.

17) 2.2.2. was provided to explain the error/bias propagation discussed/demonstrated in conclusion part. Number of equation in this section was actually cited and used in conclusion and discussion context.

18) there is. Please see line 30 page 5215.

19) comment 10 was explained: it is one stream of ensemble kalman filter. This is already published by several studies, and easily found by reference. However, it is very possible to kindly introduce that. Secondly, the aspect of ensemble kalman filter can be introduced in a final version, although those previous studies indicated in reference list already redundantly discussed/informed the same contents in several papers. However, it is very possible to kindly remind those things again in a final version.

Sakov et al., (2008) 'IS' in the reference list. It is Sakov and Oke, (2008). Only first author was chosen as a citation. Dr. Sakov is a first author as well as corresponding author in this publication. Please see line 25 on page 5215.

Reichle et al. (2008) 'IS' not in the reference list. Please see line 20 on page 5215.

It is difficult to understand that you are saying there is a problem with reference.

20) If adding subscript i, it will be each ensemble member. However, Equation (3-1) represents a matrix form for the full ensemble. In equation (3-3), subscript i was used to differentiate the ensemble mean from each ensemble member.

By selection, those equations were extracted to explain the peculiarity of deterministic ensemble kalman filter (why specifically deterministic method was preferred), as you asked it is unclear what deterministic EnKF is, and how it is different from traditional ensemble kalman filter. Equation (3-1) is needed to explain what was cancelled.

21) it is defined in line 19 on page 5205. You can refer to equation (16) in Sakov et al. (2008).

22) How ensemble was generated is described in line 16 on page 5206. NDVI is an input to perturb the aerodynamic roughness height. For relationship between them, please see method 2.2.1. and AC # 16 in this posting. Relationship and propagation was described on page 5203.

23) page. 5208, rows 1-4 is Gaussian error propagation as a variance. Please refer to Marx et al.,

(2008). NRR_H and NRR_{LE} are defined in line 18 on page 5206. NRR H stands for NRR of sensible heat H, while NRR LE stands for NRR of latent heat LE.

24) DEnKF is a type of EnKF. EnKF in the text obviously implies DEnKF, in specific. It is the only one kind EnKF used in this entire paper. No other types of EnKF was introduced.

25) It was demonstrated in Figure 6. Let me add some comments on the title of graph with 'the impact of z0m in heat flux'. Before and after calibration over z0m, please see Figure 1 and Figure 5. It showed a sensitivity.

26) RC) Again, this sentence needs to be demonstrated. You need to show results (graphs for instance) and explain better your results.
AC) it is demonstrated by equations in line 2-4 on page 5208. It is simply a series of variance by common statistics definition (e.g. MS office EXCEL).

RC) In this sense, did you make any sensitivity analysis of the model? If yes, what type of analysis?
AC) It is Figure 6, showing the sensitivity (differences in heat flux before and after aerodynamic roughness adjustment). For a comparison between aerodynamic roughness before and after adjustment, please refer to figure 1 vs figure 5.

Global multivariate has nothing to do with current study. This is univariate data assimilation.

RC) Again, first of all you need to show the calibration and validation of the model.
AC) Results spent the entire space to show calibration and validation (using Naqu heat flux and landriano station eddy covariance data etc.).

27) Again, two equations are error propagation as in variance as explained in comment # 23 as well as # 26. Equation number was omitted by HESS edition for some reason.

28) Spread of ensemble pool was arithmetically assessed, quantified and demonstrated as a number (NRR) on page 5207 line 1. Visual demonstration that you are requesting is considered less meaningful and not enough.

All the data used in this study is 2006. There is no 2005 (when eddy covariance is available) data used. The objective of this study is to make a reasonable estimation of aerodynamic roughness when eddy covariance is not available in 2006. Validation against the eddy covariance was conducted in 2005 over Naqu site, and 2006 over Landriano station. Furthermore, comparison between BREB and eddy covariance is not only explored by van der Velde et al. (2009), but BREB was also used in Tibetan plateau as a long term by several other studies (Lu et al., 2012, Liu et al., 2009, Oku et al., 2008). There is no need to stick to 2005 data in April as BREB was applied to different time in September in the same year in van der Velde et al. (2009). More than anything, the error of BREB is neither time-sequentially propagated nor amplified when it is extended to a long term. Level of vapour pressure and temperature gradient (this is a source of error rather than a time sequence) is more important as data were filtered out in methods on page 5202 and selected on page 5207. It is also doubtful if a period of 120 days (experimental period in this study) can be called as a long-term. For your information, Moradkhani et al (2005) carried out their experiment with data for three years. However, several input parameters were not converged in data assimilation for some reason. We are wondering if there is a logical ground or reference that you call a length of experimental period (long term or not) into a question. Then, we will seriously review the literature that you refer to.

Sensible heat flux before data assimilation was reported as 65 W/m2 as written in line 10 on page 5208.

Citation)

Lu, Hui; Koike, Toshio; Yang, Kun; Hu, Zeyong; Xu, Xiangde; Rasmy, Mohamed; Kuria, David; Tamagawa, Katsunori. (2012). Improving land surface soil moisture and energy flux simulations over the Tibetan plateau by the assimilation of the microwave remote sensing data and the GCM output into a land surface model. International Journal of Applied Earth Observation and Geoinformation, Volume 17, p. 43-54.

Liu, S., Li, S.-G., Yu, G.-R., Sun, X.-M., Zhang, L.-M., Hu, Z.-M., Li, Y.-N., and Zhang, X.-Z. (2009). Surface energy exchanges above two grassland ecosystems on the Qinghai-Tibetan Plateau, Biogeosciences Discuss., 6, 9161-9192, doi:10.5194/bgd-6-9161-2009,

OKU, Y and ISHIKAWA, H. (2008). Land surface energy budget over the Tibetan plateau based on satellite remote sensing data. Advances in Geosciences Vol. 16: Atmospheric Science.

29) aerodynamic roughness is propagated by NDVI perturbation. Please refer to as method 2.2.1. on page 5203.

30) initial aerodynamic roughness.

31) In figure 4, on Julian day 132, z0m= 0.003 m (the highest number on y-axis) was the most frequent so that this number was accepted as aerodynamic roughness height on Julian day 132, while z0m= 0.015 m was the most frequent (the highest number on y-axis) so that this number was accepted as aerodynamic roughness height on Julian day 163. Similar approach can be found in previous study (Yang et al., 2008).

32) z0m for original SEBS was demonstrated in Figure 1, while heat flux of original SEBS was demonstrated in Figure 6. Calibrated z0m was shown in Figure 5.

33) Yes, It is a Julian day of 2006. It is very possible to change it to DOY.

34) Soil moisture in Figure 7 indicates a top layer surface. Previous study (Li et al., 2010, Margulis et al., 2002) employed latent heat as an indirect proxy as well as consequence of surface soil moisture by its correlation. Rain fall -> Surface soil moisture -> Latent heat.

"Consequently, LE_soil indirectly reflects the availability of water within the vegetation root-zone. Since LE_soil also contains information about the availability of surface soil moisture (due to the direct impact of surface soil moisture on soil evaporation levels), it can be viewed as an effective proxy for the integrated availability of both surface- and root-zone soil water" (Li et al., 2010).

"It is difficult to obtain ground truth measurements for checking the accuracy of soil moisture estimates in subsurface layers. One alternative is to compare observed latent heat fluxes to the values estimated by the ensemble filter. These fluxes provide a useful aggregate measure of profile soil moisture." (Margulis et al., 2002).

35) It was used in line 1 on page 5210. This equation will be shifted to line 3 on Page 5210 in the end of explanation.

36) time-series bowen ratio heat flux was already indicated as blue dot in Figure 6.

37) Repetition was avoided. It was expressed as Bowen ratio, which was quantified as a number in following lines.

38) It is really difficult to agree to this at all. This is one of the key findings that this study tried to contribute to, although you could still be aware of this if reading the AC to the 1st reviewer's comment. This calibration is meaningful and useful because this could adjust the energy partitioning. It was a curiosity of this research paper whether sensible heat stays as a dominant energy even during or after the Monsoon period as generalized in previous review study (Ma et al., 2009). If sensible heat is 'always' exclusively a dominant energy, that implies a significant drought.

39) Precipitation and surface soil moisture in Figure 7 were field measured, not estimated by energy and water balance. Previous study (Ma et al., 2009) already reported that energy balance is not closed. Energy balance is not required to be closed since several other sources such as carbon

photosynthesis flux, crop enthalpy and soil surface heat flux are also unknown in several cases.

40) This site was briefly described in line 3-5 on page 5211. Landriano data were used to add more climatic versatility to this approach. This will confer readers a degree of flexibility in establishing the operational frame for their own site. Following additional reference will be added in a final version.

Baroni, G., Facchi, A., Gandolfi, C., Ortuani, B., Horeschi, D., and van Dam., J. C. (2010). Uncertainty in the determination of soil hydraulic parameters and its influence on the performance of two hydrological models of different complexity. Hydrol. Earth Syst. Sci., 14, 251–270.

However, if appearance of landriano data hinders the consistency of this paper, deletion will be seriously considered.