Answer to all the points from the referee#4

General comments:

Questions and comments (Q&C): The Tibetan Plateau is a very unique place. Understanding the surface energy partition for the area, especially differences between the area and the other area with the similar land surface type, is important. The subject of the manuscript fits ACP well. However, the manuscript is very similar to their early paper published in the same journal in 2011, i.e., the same methodology (even though “a new parameterization method” was claimed in the abstract), the same Fig.1, the almost same Fig. 2, and the similar Figs 3, 4, 5, and 6. The title of the manuscript is on LE, but the authors did not even change their symbols for the definition of APD in this manuscript and copied the one used in their 2011 paper, where the focus is on H. The only difference is that the authors introduced another satellite dataset, AVHRR to compare it with the satellite dataset from MODIS. Even for this difference, the authors failed to explain why the MODIS dataset is better than the AVHRR one. I do not see significant scientific values in this manuscript compared to their 2011 paper. Therefore, I suggest rejection of the manuscript at the current form.

Answer (A): Thank you very much for your key and very good comments and suggestions. We have revised the manuscript in detail according to your comments and suggestions. You are right, Fig.1 is same as our 2011 paper, and the procedure of Fig.3 is mostly same as our 2011 paper. We have deleted the former Fig.1 and Fig.3 in the revised manuscript now according to your comments and suggestion. We have three main improvements in the revised submission compared with our 2011 paper now: (1) Evapotranspiration (*ET*) between the land surface and atmosphere of the Tibetan Plateau play an important role in the Asian Monsoon system, which in turn are major components of the energy and water cycles of the global climate system. Therefore, the determination of *ET* over the Tibetan Plateau is very important in the study of energy and water cycle over this area. We focus on the *ET* in the revised manuscript now. We introduce “evaporative fraction” Λ as a new index for *ET* in the revised manuscript instead of determining *ET* directly due to two reasons: (a) evaporative fraction Λ is more suitable as an index for surface moisture condition than *ET*. (b) evaporative fraction Λ is useful for scaling up instantaneous observations to longer time periods (detail statements can be found from Page 4 to Page 5 in the revised manuscript). We want to find a better determination method and satellite data for the *ET* over heterogeneous landscape of the Tibetan Plateau. (2) Four images of AVHRR data (17 January 2003, 14 April 2003, 23 July 2003 and 16 October 2003) and eight images of MODIS data (17 January 2003, 14 April 2003, 23 July 2003 and 16 October 2003; 30 January 2007, 15 April 2007, 1 August 2007 and 25 October 2007) were used in the revised manuscript, and only four images of MODIS data (30 January 2007, 15 April 2007, 1 August 2007 and 25 October 2007) were used in our 2011 paper. Same day images of AVHRR and MODIS data in 2003 were used to find which satellite data is better for the determination of the evaporative fraction over heterogeneous landscape of the Tibetan Plateau. We found MODIS is better than AVHRR in the present revised manuscript through the validation of the derived results by using field observation in the Tibetan Plateau. Then, we use four more images of MODIS data (30 January 2007, 15 April 2007, 1 August 2007 and 25 October 2007, the data is used same as in Ma et al.,(2011)) in the revised manuscript for the more validations of method due to more validation sites set up in 2007 over the Tibetan Plateau. And we would like to demonstrate again that the method using MODIS and in-situ data to determine the evapotranspiration (*ET*) over the Tibetan Plateau is the better one. (3)Surface and atmospheric parameters (surface reflectance, surface temperature, surface emissivity, and vegetation coverage etc.) are very important in the determination of surface heat fluxes and *ET* in Fig.3 (in former manuscript).The different satellite data was used in the present revised manuscript and in our 2011 paper. It means that in Fig.3 (in former manuscript)the different determination procedures of surface reflectance, surface temperature, surface emissivity, vegetation coverage, downward shortwave and long-wave radiation etc. were used in the two papers.

Fig.1(in the present revised manuscript) in the manuscript is to demonstrate that evaporative fraction Λ is useful for scaling up instantaneous observations to longer time periods(detail statements can be found from Page 4 to Page 5 in the revised manuscript), and Fig.1(in the present revised manuscript) is a new one and it cannot be found in 2011 paper.

I am sorry, “H” in Eq.(3) for the determination of *APD* in the former manuscript is not sensible heat flux, it is a common symbol. Therefore we used it in former manuscript and our 2011 paper. We revised our manuscript (change “*H*” to “*V*”) now according to your comments to avoid the confusion (see new Eq.(3) in the revised manuscript, please).

Q&C: Here are some suggestions if the authors want to resubmit the manuscript. Some of the issues are relevant to their 2011 paper too.

A: Thank you very much for your comments and suggestions in detail. We have revised our manuscript in detail according to your following comments and suggestions (see the revised manuscript, please).

Q&C:

1) If the difference between the two satellite datasets is the focus of the paper, please say so from the title and describe how they are different in the text. The authors already demonstrated their strategies to derive H and LE in their 2011 paper.

A: Thank you very much for your comments and suggestions. You are right, the difference between the two satellite datasets is the focus of the paper. We have revised the title and our manuscript according to your comments and suggestions (see the revised title and manuscript, please).

Q&C:

2) In terms of the methodology, the authors need to clearly state the limitation of the method. As I understand the approximately constant EF is only valid during daytime under clear sky conditions with no serious weather conditions and soil moisture limitations. Theoretically EF has to depend on land surface types. To me, it is much valuable to describe how good the invariant EF method works for different types of land surfaces. Can the authors describe just briefly how well the calculated downward solar radiation compared to the observed one if there are any?

A: Thank you very much for your comments and suggestions. You are right, the approximately constant evaporative fraction Λ is only valid during daytime under clear sky conditions with no serious weather conditions and soil moisture limitations. Fig.2 (Fig.1 in the present revised manuscript) shows that evaporative fraction Λ depends on the land surface types. We have already stated this characteristic in the revised manuscript now. The downward solar radiation (or downward short-wave radiation) have calculated and compared with the in-situ data in former papers. Here we just give the comparison of net radiation flux R_n.

Q&C:

3) To compare the H and LE measured in the fields with the satellite derived ones, the authors need to explain the necessary details, such as what kinds of measurements are available at all the towers? How are the sensible and latent heat fluxes were derived? From Fig.3, it looks like the bulk formula is used to derive those quantities and no direct eddy correlation measurements are available. If so, how are all the relevant

parameters, such as z_oh and d0, derived at each tower? What does each dot represent in Fig. 6 for the important comparison? What is the averaging time? There is a significant number of points in Figs. 6c-d that EF is larger than 0.5. They seem to be high for the NDVI map demonstrated in Zhong et al. (2010).

A: Thank you very much for your very good comments and suggestions. You are right, we should explain sensible heat flux *H* and latent heat flux λE measured in the fields in details. We have listed all the stations and their relevant information (land cover type, elevation, latitude, longitude, instruments, observation items etc.**)** in detail in a Table according to your comments and suggestions (see Table 1 in the present revised manuscript). There are direct eddy correlation measurements in seven stations (BJ, NAMOR, QOMS, SETS, Amdo, Maqu and Haibei). They cover almost all kinds of "tile" (land cover type) of the Tibetan Plateau. Therefore, we can derive all the relevant parameters in the former Fig.3 for the "Tile approach" to determine sensible heat flux. You are right, bulk formula has been used for the determination of sensible heat flux H and latent heat flux λE etc. in the normal towers stations (three level AWS stations, D105, NPAM and ANNI in our research). We have pointed these issues in the present revised manuscript now. Each dot in Fig. 4(in the present revised manuscript) represents the measurement evaporative fraction Λ in the stations and derived Λ from parameterization method when the satellites overpass the Tibetan Plateau. As for significant number of points larger than 0.5, I think it is correct in summer time. The latent heat flux is much larger than sensible heat flux in summer time from the field observation. Therefore some points of the measurement evaporative fraction Λ in the stations (Fig.1 in the present revised manuscript) by suing Eq.(1) and derived Λ from satellite parameterization method (Fig.3 in the present revised manuscript) by using Eq.(2) are larger than 0.5. We can also find some pixies of NDVI in summer(July, August and September) are larger than 0.5, and some of them are from 0.7 to 1.0 in Fig.5 of Zhong et al.(2010).

Q&C:

4) Please use symbols consistently. If EF is the one that the authors would like to focus on why introduce Λ?

A: Thank you very much for your comment and suggestion. We use Λ as a normal symbol of evaporative fraction (EF). It is easy to use Λ to instead of EF in the equations and figures. We change all the EF to Λ according to your comments and suggestions in the present revised manuscript now. Surely, we have used symbols consistently in the revised manuscript now.

Q&C:

5) For clarity, it is better to list all the stations and their relevant information in a table.

A: Thank you very much for your comment and suggestion. We have listed all the stations and their relevant information in Table 1(see Table 1 in the present revised manuscript, please).

Q&C:

6) Please define all the symbols used in the manuscript. I assume LT in Fig. 3 is local time? The units should be hours? Also, how were all the data derived in Fig.3? Are they averaged diurnal variations over a period of time or for a particular day?

A: Thank you very much for your very nice comments and suggestions. I think "Fig.3" should be "Fig.2" in your comments, right? If so, you are right, LT in Fig.2 is local time, and the units are hours. I have pointed out these issues in Fig.1 (Fig.2 in the former manuscript) in the present revised manuscript now. Evaporative fraction Λ in Fig.1 (in the present revised manuscript) are derived from sensible heat flux H and latent heat flux λE measured by eddy correlation method in four stations. They are averaged diurnal variations over 16 clear days in summer time, three days in June, four days in July, five days in August, and four days in September. It has also been pointed out in the present revised manuscript now.