Response to Interactive comment on “Estimation of evapotranspiration in the Mu Us Sandland of China” by *S. Liu et al.*

Dear Anonymous Referee#1:

First of all, we greatly appreciate your careful work and very useful suggestions. We will try to take advantage of your advice for improving the manuscript. For an easier comprehension, your comments are also reported. We respond below in blue to your comments item-by-item.

Major concerns:

Referee #1: It is not clear for the review what is the exact scientific issue to be solved in the study. As stated in the introduction, a number of methods for ET estimation have been available. The necessity to develop a new ET model should be clarified. Also, the authors should clarify whether their new method is specifically developed for the study area or it is also applicable to other regions. In a word, exact and sufficient motivations for this study should be provided.

In this paper, regional ET was estimated with remote sensing and meteorological data by the complementary relationship approach in Wushen county and its temporal and spatial distribution was analyzed. The advantages of the complementary relationship approach we used in the manuscript lies in its input solely depending on widely available meteorological data, while other ET models need soil moisture, stomatal resistance, and aerodynamic resistance etc., which are difficult to be obtained in practice. Moreover, the applicability of the complementary relationship in different climate regions has been validated by Xu and Singh (2004), Liu et al.(2006), Virginia Venturini (2008) as well as Cory Pettijohn and Salvucci (2009), we have mentioned it in the first section ” Introduction” on Line 12-17 in page 5979. We will perfect our research significance in the revised manuscript.

Referee #1: Rn and G are crucial forcings in Eq.(4). The calculation of solar radiation and net longwave radiation should be very careful.

a. For solar radiation estimation from sunshine duration, it is surprising that the determination correlation (r^2) between observations and estimations by Eq. (6) is as low as 0.7 at the two sites in this region. According to my experiences, this coefficient after a local calibration is generally higher than 0.9. I doubt that some observation data of solar radiation are suspected to be erroneous, as it is well known that CMA (China Meteorological Administration) radiation data are not so reliable before 1994 (Shi et al., 2008, Data quality assessment and the long-term trend of ground solar radiation in China, J Applied Meteorol. Clim, 47, 1006-1016, doi:10.1175/2007JAMC1493.1). I suggest the authors checking the data quality before calibrating coefficients in Eq. (6). Meanwhile, it is not explained how the regressed coefficients in Table 2 are extended from two sites to 2D space. Also, it is reminded that the coefficients cannot be linearly interpolated as they are dependent on elevations and climate regimes.

First of all, thank you for your remind on the determination correlation of Eq.(6), we will firstly check the quality of radiation data before calibrating the coefficients in Eq(6).

With respect to the interpolation of the regressed coefficients in Table 2, as our study area lies in a limited region, with an total area of $4*10^4 Km^2$, both the climate and the elevation are uniform in the study area, so *a* and *b* fitted from two sites are considered as suitable for the whole study area. However, sunshine percentage *S* in Eq.(6) is a variable changed with local weather condition which can be obtained from meteorological stations in and around Wushen county, thus (*a*+*bs*) was interpolated to pixel scale from weather stations using Kriging method. As a result, total radiation can be calculated from astronomical radiation and (*a*+*bs*) of each pixel.

b. The net longwave radiation calculation presented in this paper can be risky. The parameters in Eq.(7) were obtained from a very small number of stations. The authors should show its applicability at the specific area of interest. Or, I would suggest the authors to consider Crawford and Duchon (1999, J. Appl. Meteorol. 38, 474–480.) model, whose inputs are identical to Eq. (7). At least, this model without any local calibration has been proven to be reliable in recent studies (Choi et al., 2008, Geophys. Res. Lett. 35, L20402, doi:10.1029/2008GL035731.; Yang et al., 2009, Agric Forest Meteorol, doi:10.1016/j.agrformet.2009.08.004).

Thank you for your useful suggestion, both the model we used in the manuscript and you mentioned will be compared and validated using ground measurements, then a preferable way of net longwave radiation calculation will be determined.

Referee #1: Validation issue. The authors only show the validation of ET, however, the calculation of radiation and G should be validated as they are important inputs. Their errors might be the cause that ET is slightly under-estimated. Moreover, the ET validation itself is too limited (only two monthly-mean values are used) and the authors should consider more data for the validation, if possible.

We agree with the referee, that (Rn-G) is a significant parameter in the calculation of ET. At present, we are trying to validate Rn with ground measurements during the period of ET estimation. As daily/monthly G is close to zero, so its erroneous on monthly ET can be negligible.

Meanwhile, though ET measurement in sandland is scarce, we will try our best to look for more ET data during our study period.

Referee #1: The authors analyzed the ET spatial pattern and ET temporal variations. However, the implication of these pattern and variations for water resources and land use management are not discussed. In other words, the scientific significance of these analyses should be presented.

It seems that we have not given enough emphasis on the implication of ET spatial and temporal pattern, though we have pointed out its function on Line 14-15 in page 5990. In the revised manuscript, we will give more discussion about the research significance and practical application.

Minor comments:

Referee #1: s in Eq. (2) is the ratio of diffuse radiation to the global radiation. How to determine it?

In our study, *s* in Eq.(2) is deemed as an empirical value. In our revised manuscript the ratio will be determined by long term ground measurements according to different seasons.

Referee #1: Citing (Liu et al., 2006) for Eq. (3) can mislead the readers as it was proposed by Bouchet (1963) instead of Liu et al. (2006)

We agree with the referee, and in the revised manuscript, the citing reference will be modified from Liu et al. (2006) to Bouchet (1963).

Referee #1:Is there any pre-requirement (climate regime, vegetation) to apply Eq. (4)?

As mentioned in the manuscript, the complementary relationship of Eq. (4) has been tested by different authors, so its applicability in various climate regions and surface layers are reliable (Morton, 1983, Qiu et al., 2004, Xu and Singh, 2004). However, before applying Eq.(4), Priestley-Taylor coefficient has to be predetermined according to climate regions and research period.

Referee #1:Is the coefficient of c in Eq.(10) calibrated in this study or by a reference. If it is in this study, what data are involved in the calibration?

The coefficient of *c* in Eq.(10) was calibrated according to the reference of Prere and Povov(1979), which was established for arid region to compensate the underestimation of potential ET. In this equation, *c* was determined according to monthly maximum and minimum temperature.

Referee #1: Eq.(11). I guess the RHS should be divided by n.

We agree with the referee, this is a clerical error in the manuscript, “n” will be added in our revised manuscript.

Referee #1: P5987, L24: remove “trend”, as it is just variation

Thank you for your suggestion and the word “trend” will be removed in the revised manuscript.

Referee #1: Table 3: The legend is confusing. These values are merely interpolated from other stations to Wushen rather than determined from data within Wushen.

It is really a bit confusing. The legend of Tab.3 will be corrected in the revised manuscript.

Referee #1: It is informative to include precipitation anomaly in Figure 5.

In Figure 5, precipitation will be included in the revised manuscript.

Referee #1: Figure 6 shows E values in Jan., Feb., Mar., Nov., Dec. are zero. Is this true or due to some algorithm adjustment?

In Figure 6, the amount of ET in Jan., Feb., Mar., Nov. and Dec. all have values, though are closed to zero. As we mentioned in manuscript on Line 9-11 in page5988: “During winter and spring (Nov., Dec., Jan., Feb., and Mar.,), the ET was extremely small due to withered vegetation, low air temperature and seasonal freezing of soil water.” The monthly air temperature and monthly soil temperature at depth 0cm are both less than zero degree in these months. So the low air/ surface temperatures result in fewer evapotranspiration in non-growing seasons.

Referee #1: The coefficient between P and ET looks low for a dryland. In some years, ET is even larger than precipitation. Again, is this true or due to the algorithm used?

As we mentioned in our manuscript, ET in non-growing seasons (Jan., Feb., Mar., Oct., Nov., and Dec.,) are very small due to low air/ surface temperature, withered vegetation, and the precipitation is not the only determined factor of decreased ET, so the correlation coefficient between precipitation and ET is low. In our revised manuscript, the growing season will be chosen for the analysis of correlation coefficient between precipitation and ET.

The month when ET is larger than precipitation only exists in Sep., within this month, though precipitation and air temperature decrease, the grass and crops are still needs much water to mature, which can be derived from soil(Zhou et al., 2008), thus ET is larger than precipitation.

Reference

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