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Hydrology and Earth System Sciences Discussions

Interactive comment on "Estimation of evapotranspiration in the Mu Us Sandland of China" *by* S. Liu et al.

Anonymous Referee #1

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The paper presents a method to estimate evapotranspiration (ET) for Mu Us sandland in China. Such estimation is, of course, a very important topic for land use and water resources management for this dryland. However, the paper needs substantial revisions before considering its acceptance.

Major concerns:

(1) It is not clear for the review what is the exact scientific issue to be solved in this 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.

(2) 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 (r2) 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.

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).

(3) 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

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consider more data for the validation, if possible.

(4) 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.

Minor comments:

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

(2) 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)

(3) Is there any pre-requirement (climate regime, vegetation) to apply Eq. (4)?

(4) 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?

(5) Eq.(11). I guess the RHS should be divided by n

(6) P5987, L24: remove “trend”, as it is just variation

(7) Table 3: The legend is confusing. These values are merely interpolated from other stations to Wushen rather than determined from data within Wushen.

(8) It is informative to include precipitation anomaly in Figure 5

(9) Figure 6 shows E values in Jan., Feb., Mar., Nov., Dec. are zero. Is this true or due to some algorithm adjustment?

(10) 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?

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