Response to referee #1's comments

Major revisions:

(1) We have rewritten section 4.2 to clarify our target and relevant details;

(2) All models are re-run with some parameters derived from observed radiation budgets and soil temperature profiles. These parameters are surface emissivity and albedo at all sites and thermal diffusivity at alpine desert sites. Thermal diffusivity at alpine meadow sites is variable due to frequent rainfall events and thus default values in the individual models are used. With these adjustments, all models could simulate nocturnal surface temperature in a reasonable accuracy for the desert sites, while the daytime surface temperature is not good if the excess resistance is not included or not reasonably parameterized.

Response to General comments

Comment:

There is quite some arbitrariness in the choices for sites, periods etc. For instance, the evaluation of NCEP, JMA etc with local data shown in Fig 1 considers a different time period than was used for the modeling study (May-Sep 1998). This should be made consistent.

Response:

We do not intend to compare our modeling results with the GCM evaluation for a specific year. Our major purpose here is to show the discrepancies among the models, by which we explain the motivation of this study.

The major consideration to use different periods for the modeling study and for the GCM evaluation is the data availability. Regarding the modeling study, we need comprehensive observed data, which were collected in 1998 through GEWEX/GAME-Tibet project. Regarding to the GCM evaluation, we used the convenience provided by CEOP project. Unfortunately, observations in Tibet could not be well implemented during CEOP period (2002/9-2004/12). On the other hand, 1998 is not a period of CEOP project, and therefore, the data required for the GCM evaluation (JMA, ECPC, UKMO) are not easily accessible for this year. Though to search for the GCM data of 1998 is not absolutely impossible, the difficulties to find the data is indeed the major reason why we used the readily available GCM data collected during the CEOP period.

Comment:

Rather than showing a number of "representative days" (which are actually quite variable) in figs 3-5, you should consider to plot a mean diurnal cycle for relevant dry/wet periods of all quantities. This makes the comparison more informative and less arbitrary

Response:

This is indeed a good suggestion! We have followed this comment and redrew Figures 3-5, in which monthly-mean diurnal variations of surface temperature and heat fluxes are shown. Note old Figure 3 and Figure 4 are merged in the new Figure 3, and the new Figure 4 shows the case at another alpine meadow site (MS3478), which was not shown in the first version. New Figure 5 and Figure 6 are shown similarly, respectively, for the two desert sites.

Comment:

To my opinion, the several statements that the soil moisture evolutions in the model are in error refers to fairly common and often documented knowledge on the fact that soil moisture in models is a quantity whose annual range is highly determined by the specified soil hydraulic characteristics. I don't believe that prescribing "default" soil texture parameters and initializing the models with observed soil moisture content is a consistent and honest way to run this kind of experiments. Either use local (observed) texture parameters, or allow the land models to reach a representative value of soil moisture by a sufficiently long spin-up procedure (see e.q. Rodell, M., P. R. Houser, A. A. Berg and J. S. Famiglietti, 2005, Evaluation of Ten Methods for Initializing a Land Surface Model, J. Hydrometeorology, 6(2),146-155)

Response:

The first version of our MS may have misled the reviewer. It is absolutely important to specify reasonable hydraulic parameters and well initialize a model. The relevant reference (Liang and Guo, 2003, Rodell et al, 2005) has been included in the discussion. In this study, we focus on improving soil water simulations by introducing Ross (2003) scheme into SiB2, but not on soil parameter specification and its possible effects. This scheme can handle the high nonlinearity of the Richards equation. In this paper, we do not want to fully introduce the detail of Ross scheme, but his paper is indeed devoted to developing a high-accuracy scheme. Nevertheless, the first version of our manuscript did not well present either our target or the merits of Ross scheme, and now we reformed section 4.2 and tried to give a clearer description. In addition, We have added text to read "Though partial errors in the simulated soil moisture can be attributed to specifying soil hydraulic parameters, we cannot exclude errors due to improper parameterizations for calculating soil water flow and the soil surface resistance within the dry soils, as will be discussed in Section 4.2" (P8 L16-19) and "Specification of default parameters and model initialization may cause significant errors in soil moisture, surface temperature, and surface energy budget, which is fairly common and often documented knowledge (Liang and Guo, 2003; Rodell et al., 2005) and is not the scope of this study. In this section, we present the model deficiencies that are associated with the aforementioned modeling errors, and then suggest or implement new schemes to improve the modeling." (P8 L26-31).

Comment:

Adequate references to existing literature should be given on several subjects: the above mentioned soil moisture initialization/representation is a field of research where

many citations could/should be given. Also the problem of the excess resistance is a nearly classical field going back to Mason in the eighties (see for an overview Verhoef, A., H.A.R. de Bruin and B.J.J.M. van den Hurk (1997): Some practical notes on the parameter kB-1 for sparse vegetation; J.Applied Meteorol. 36, 560-572.). More references should also be given to describe the climate at the TP (for instance, 1200 W/m^2 solar insulation should be supplied with a documented reference)

Response:
Relevant references (Liang and Guo, 2003; Rodell et al., 2005; Verhoef et al., 1997; Ma et al. 2005) have been included in the text. Frankly speaking, we are familiar with Verhoef et al.'s work, which has cited in several works of ours. Our major objective here is not to address this issue itself, as it has been addressed in a number of studies. What we really wanted to do is to implement such a scheme into a LSM to improve the land surface modeling at the Plateau surfaces.

Regarding the TP climate, the authors have spent time on summarizing its features, according to field experiments. The data (diurnal range of surface temperature, the amount of precipitation) presented in the summary are directly derived from the observational data we are using. The very high solar insulation (1200 W/m2) is also found in the data set but could be found in an earlier reference (Ma et al., 2005).

Comment:
The structure of the paper is rather ad-hoc. Please write a clear introduction section in which you outline what can be expected: (1) evaluation of TP climate with large scale climate data, (2) evaluation of the offline models, (3) evaluation of a modified model (SiB2 with adjustments). For (2) and (3) the same plots and metrics should be used.

Response:
Thank you for this suggestion! We have rewritten the last paragraph of ***Introduction***, in order to clarify the structure of the paper. The data and all simulation information, which were sporadic in sections 2 and 3 of the previous version, are summarized in Section 2 in the revised version.

Response to Specific comments

Comment:
S1292, 26: please provide a reference for this high value of solar radiation

Response:
Ma et al. (2005) has been added.

Comment:
S1294, 20: I do not think that your results are "robust", as you use default parameters in combination with local observations to initialize the models. This is normally not a

good practice, as explained above. Please prove why you consider the results as "robust"

Response:
We re-run the models using several parameters (soil surface emissivity and albedo at all sites and thermal diffusivity at alpine desert sites) derived from observations, in order to enhance the robustness of the results. This has been explained in Section 2.2 (p6 L 7-20). This specification has much improved the temperature simulation at the alpine desert sites. Land processes at the alpine meadow sites are mainly controlled by the soil vertical stratification, and we do not have enough knowledge to specify the soil parameters, for which we only address the importance of the soil stratification in this study. We are going to conduct laboratory experiments and to quantify the effect of soil stratification since this year, which would take a long time and will not be addressed in this paper.

Our identification to the model deficiencies is essentially based on field work and data analyses. (1) The soil stratifications in CE-TP are directly exhibited by the observed soil moisture. (2) The excess resistance is derived from independent data analysis in previous studies. As shown in Figure 12, we can simulate the diurnal change of surface temperature in a reasonable accuracy after implementation of this scheme in SiB2. (3) Your major concern might be the section 4.2. We rewrote section 4.2 and explained why we need to introduce Ross scheme into a LSM, which is a mathematical improvement to soil water modeling and does not concern soil parameters.

Comment:
S1295, 14: also a reference would help to justify this method of measuring skin temperature. To my feeling you get a rather arbitrary value of a surface temperature when one sensor is exposed to two different environments (buried and exposed).

Response:
This is a routine method to measure the skin temperature for the bare soil surfaces in China. In fact, at the beginning we also doubted the method of skin temperature measurement. However, after comparisons with the skin temperature converted from longwave radiation measurements, we found they are quite comparable to each other; The uncertainties between the two are about 2-3 K, which is much less than the modeling errors presented in Figures 5-6. The following figure shows an example of the comparison between the routine method-measured surface temperature (Tg_routine) and the longwave radiation-converted surface temperature (Tg_lw) for Shiquanhe site, a typical alpine desert site (see the following picture for the landscape, taken by Mr. Chen Xuelong on May 24, 2008). The surface emissivity used for the conversion is 0.90, which was determined by the condition that Tg_routine would be reliable near sunset when the difference between air temperature and soil temperature is small.
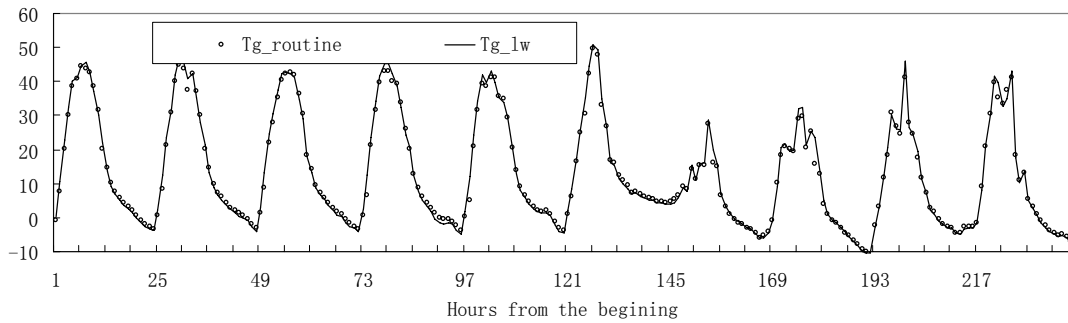
Temperature comparisons at SQH, 1998

(SQH landscape picture, taken by Mr. Chen Xuelong on May 24, 2008)

Comment:

S1296, 20: I don't understand why the K-theory in SiB2 is not consistent with the classic mixing-length theory. If it is a first order K-model, a mixing-length concept is the classical heart of the method. I also don't understand how the mixing-length model by Watanabe can "spontaneously" give bare ground roughness values. This is probably imposed to the equations. And finally, I don't understand why SiB2 cannot cope with LAI = 0, when roughness is concerned. SiB2 is used by several modelling communities in global models, and I cannot believe that serious roughness problems over bare grounds (deserts) will never have been documented before. Please consult the literature on how earlier implementations of SiB2 have dealt with this problem

Response: The K-theory is widely used to close the equations for canopy drag calculation. The core of K-theory is how to formulate the momentum transfer coefficient K_m. In SiB and SiB2, it is assumed

$$K_m \propto u \, , \tag{1}$$

where u is the wind speed.

The classic mixing-length theory assumes

$$K_m \propto z^2 \left| \frac{\partial u}{\partial z} \right|. \tag{2}$$

When LAI approaches 0, $u \propto \ln z$ for neutral condition. Eq.(1) becomes

$$K_m \propto \ln z, \tag{1a}$$

and Eq. (2) becomes

$$K_m \propto z. \tag{2a}$$

Clearly, Eq. (1a) is not identical to Eq. (1b). Therefore, the K-theory in SiB2 is not consistent with the classic mixing-length theory. The following figure shows the roughness length (z_0) changes with respect to LAI for a canopy. It is shown that z_0 calculated by SiB2 (z_0_SiB) is less than the roughness for the bare soil surface (z_0_ground) when LAI < 0.18, while the roughness length from Watanabe & Kondo scheme (z_0_Watanabe) approaches z_0_ground. When LAI is large, the two schemes produce comparable roughness lengths.
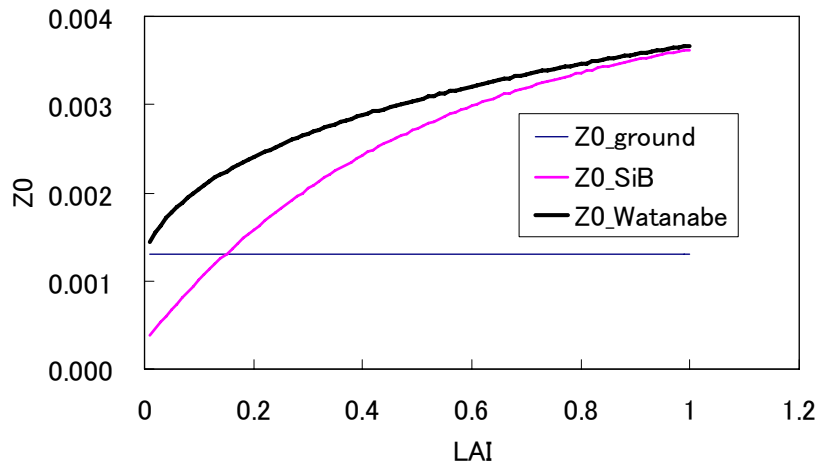
Comment:
Inspection of figure 4 tells me than SiB2 does a good job on H and a bad one on LE. This means that the sum of LE + H is presumably different for SiB and the observations. Also the other models seem to have a higher H + LE than the observations. If the observations are not reliable because of a lack of energy balance closure (often documented!) please don't use the observations or derive a measure from the observations (evaporative fraction for instance) which you do believe can be used for verification.

Response:
Regarding to the energy closure issue at Anduo site (shown in Figure 4), at least three independent studies (Tanaka et al., 2003, Yang et al., 2004, Su et al., 2006) have shown H is reliable and lE data is questionable using different analysis methods. According to your comment, we have added the reference and removed the LE

observations from the comparisons of LE (Figure 3, P6, L17-19).

Comment:

S1299, 8: The model biases are clear but very small. To me it does not justify a whole new soil moisture model. Also, the impact of the soil moisture modifications on evaporation is not demonstrated, but may be so small that you could ignore the effects.

S1299, 12-13: To be honest, I think that your main points (1) and (2) are overstatements because (a) the differences are fairly small and can have a very small effect on the surface fluxes, and (b) you can repair the differences by replacing soil hydraulic properties by better local values.

Response:

Though not clearly stated in the first version of our MS, the objective of section 4.2 is to implement mathematically improved soil water scheme (Ross scheme) and a physical parameterization for the soil surface resistance. We expect the reviewer would understand the improved section 4.2.1. However, for a complete understanding of Ross scheme, we have to go back to Ross (2003). The result with this scheme shows some potential to improve the soil water simulations, though a further evaluation is required at sites with well-calibrated soil hydraulic parameters.

Comment:

S1300, 23: It is "vd Griend" instead of "vd Grind" (also in reference list)

Response:

Corrected. Thank you!

Comment:

S1301, 11: it is unknown how you define the rh_{eq} value

Response:

rh_{eq} is the equilibrium relative humidity in the air space of the soil. It is calculated following Philip (1957):

$$rh_{eq} = \exp\left(\frac{\psi(\theta_{\text{sfc}})}{R_v T_g} g\right)$$

where $R_v = 461.5 \, \text{J K}^{-1} \, \text{kg}^{-1}$, $g = 9.81 \, \text{m s}^{-1}$,

It is given in section 4.2.2.

Comment:

The model description in this section is not very clear. I don't know what the "computational nodes" are (what kind of grid do you use), and many equations seem

very well-known to me and nothing new (e.g. Eq 3). Also you cannot reduce uncertainty by just merging two terms into one equation (S1302, 9)

Response:
This has been clarified in the foregoing response. We added a new figure (Figure 9) to delineate the computational nodes.

Comment:
Section 4.3: your new excess resistance gives better surface temperature during daytime, but leaves the big bias during nighttime unresolved. Why? Can be a very important term in the daily total energy balance. Why didn't you use CoLM for this exercise, which seems to have a better nighttime temperature result.

Response:
The differences in nighttime temperature in the individual models are caused by different parameterizations of soil thermal properties. Now we used observation-derived thermal diffusivity for the desert sites in all models, and the discrepancies in simulate Tsfc become much smaller (see Figures 5-6).

Comment:
S1303, 23-17: replace this section by simply stating that the higher sensible heat fluxes are consistent with reduced longwave cooling and higher net radiation amounts (during daytime)

Response:
We have revised it following this suggestion (P14, L15-19).

Comment:
S1304, 13: I don't think that a vertically stratified soil column with higher porosity near the surface due to organic matter is "special while widely occupied": widely occupied yes (but not only in the TP, it can be found in any forest and heavily vegetated area), but not special. And you do not convince me from your results that "many more experiments are needed to improve the models"

Response: This soil stratification in the CE-TP should be addressed for the following reasons. First, the soil stratification in the CE-TP is very significant compared to that observed in other regions. Table 3 shows soil texture and parameters obtained from laboratory experiments of soil samples taken at Anduo sites. It is clear that the bulk density of the topsoil is nearly half of the deep soil and the soil porosity in the topsoil is much higher than that in the deep soil. Second, the topsoil is of significant importance for the land-surface interactions, because high-level radiation over TP is not damped by vegetation and thus the topsoil directly and strongly interacts with the atmosphere. Though SOMs also occur in forest and heavily vegetated areas, heat exchange and evapo-transpiration mainly occur in the canopy, whereas the exchange

with the topsoil and the air is rather weak

This paragraph has been added in the revised manuscript (P9 L6-15).

In addition, we stated that "Though the topsoil interacts with atmosphere directly (due to short vegetation) and strongly (due to high radiation), we have limited knowledge on its hydraulic and thermal properties. Future studies should address this issue so as to develop a proper parameterization for the topsoil parameters." (P14 L31 – P15 l2).