Hydrol. Earth Syst. Sci. Discuss., 6, C194–C198, 2009
www.hydrol-earth-syst-sci-discuss.net/6/C194/2009/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Some practical notes on the land surface modeling in the Tibetan Plateau" *by* K. Yang et al.

Anonymous Referee #1

Received and published: 2 April 2009

General comments

The paper describes a model intercomparison study for two contrasting sites on the Tibetan Plateau. As a land surface modeller not familiar with the climate characteristics of the Tibet Plateau (TP) I really encourage this kind of studies and publications. However, the manuscript needs to be restructured considerably in order to be truly useful for the modelling community (which seems the main motivation for the paper) and to be acceptable as a scientific paper. In particular the following elements should be improved:

- 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 modelling study (May-Sep 1998). This should be made consistent.

- 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
- 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)
- 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/m2 solar insulation should be supplied with a documented reference)
- 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.

- And it should be motivated well why the modifications are necessary. As stated before, the wrong soil moisture in the models can probably be repaired effectively by prescribing adjusted values of wilting point/field capacity. I am not at all convinced that you need a new soil model, as outlined in section 4.2. And I am not convinced that this new model is actually an improvement. For instance, the clear diurnal cycle seen in the observations is still not present in the model simulations, which means that also this new model is not doing a very good job
- the title of the manuscript should be changed into somewhat like "Comparison of 3 offline land surface schemes with observations at the Tibetan Plateau"

Specific comments

In addition to the general comments on the structure of the paper, I have some specific comments:

- S1292, 26: please provide a reference for this high value of solar radiation
- 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"
- 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).

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

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

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 1291, 2009.