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Hydrology and Earth System Sciences Discussions

Interactive comment on "Turbulent flux modelling with a simple 2-layer soil model and extrapolated surface temperature applied at Nam Co Lake basin on the Tibetan Plateau" *by* T. Gerken et al.

T. Gerken et al.

tobias.gerken@uni-bayreuth.de

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The authors would like to thank reviewer No. 2 for the constructive review. We will address his comments below and improve the manuscript accordingly.

1 Major Comments

- RC: “The manuscript states that the method will be used in high-resolution circulation models but requires actual soil temperature observations which are not readily available or have to be installed for a particular purpose. Does this limit the application of this method? Even if you see improved results from using actual soil temperature observations, why was it not a purpose of this study to assess how using other data sources for the soil temperature observations (which are more readily available and easy to use) would affect the results? I’m concerned that the results here do not have tangible applicability beyond field campaigns (where such observations may be available or installed) or very limited study regions where ground-based soil temperature observations are available? Can the authors address this? If that is the main purpose of the hi-res circulation model, it might be helpful to make that clear to the reader”
  → Reviewer No.2 is correct to state that our approach is mainly applicable to locations where data of the state of the soil is available from field observations, such as measurement campaigns, field stations or remote sensing. There is an increasing amout of such data available on the Tibetan Plateau. The focus of our research is the investigation of how field data such as observations from complex environments can be incorporated into a model and the to exploring the sensitivities of these environmental systems. The current work is part of this larger research theme. We agree that for larger systems (ie. the regional or continental scale then additional methods should also be considered (ie. using remotely sensed surface temperatures and soil moistures for model initialisation). We do not think that this conflicts with the purpose of this paper which is demonstrating the feasibility of a rather simple approach.

- 10279,9: “Why were only 4 days chosen for the analysis? This is an extremely limited dataset, and if valid reasons exist for the selection of a such a limited

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dataset they need to be clearly presented to the reader, along with any potential disclaimers throughout the manuscript on the potential issues that such a limited dataset presents to the conclusions drawn throughout the results section?"
→ The dataset used as a basis for this study is derived from a field campaign in a very remote location and is quite limited. We have selected 4 days that encompass a variety of weather conditions, in order to show that our approach works. We feel that those 4 days give a fair assessment of what the model can and cannot do. We will give an explanatory sentence in the revised manuscript on why we chose 4 days. Proposed: " Due to the limited data set derived from the field campaign we restricted our analysis to four days that encompass most weather situations."

- 10280, Section 3: "How is the ATHAM model different from high-resolution mesoscale numerical weather prediction models (such as WRF and RAMS)? These models have very sophisticated models and can be run on similar scales of motion (sub 1-km), what makes ATHAM different than these models, and if differences do not exist, why not consider these other models?" → ATHAM is a model that was specifically designed for high resolutions (order 100s of meters). We aknowledge that RAMS and to some extent WRF are also used on sub-kilometer scale, but to our knowledge at there are several problems with running WRF on with a too high spatial resolution. ATHAM also incorporates the concept of active tracer. This means that all atmospheric tracer are consistently formulated within the prognotic equations and thus contribute to mass, heat capacity, gas constant etc. of the mixture. ATHAM can be used to run different simulations in 2 or 3D from very idealized setups with few processes beyond transport to more complex simulations including surface processes, radiation . . . In addition to this ATHAM is not the main focus of this work. Our intention was to successfully demonstrate the big effect a simple approach can have on the simulated fluxes.

- 10281,9: "Please add equation for LE . . . "

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→ equations will be added: "The latent heat flux is derived in a more complex manner from bulk soil evaporation (EV) and a canopy resistance approach estimating plant transpiration (TR):

$$EV = \left(\rho \frac{f_h q_s - q_a}{r_s + r_a}\right) \times \exp(-0.7LAI) \quad (1)$$

$$TR = \frac{\rho \Delta q_a}{r_c + r_a}, \quad (2)$$

the relative humidity of soil air (f_h), saturation water mixing ratio at surface temperature (q_s), atmospheric water vapor mixing ratio (q_a), soil and aerodynamic resistance (r_s, r_a), leaf area index (LAI) and canopy resistance (r_c) calculated by the vegetation model component."

- 10285,6-8: " If the model is so sensitive to the initial initialization of soil temperature,why was a careful analysis of the sensitivity to errors in soil temperature measurements not included in the manuscript? Please provide motivation for its exclusion." → The focus of our manuscript was to demonstrate that our approach is capable of improving the model rather than exploring all the sensitivities of the changed version. Our group has a vast experience of minimising errors in soil measurements (Liebethal et al., 2005) and minimising soil model errors (Liebethal and Foken, 2007) that were also confirmed by our Chinese colleges (Yang et al., 2009) Fig. 4 was supposed to show how different choices of initial parameters will alter the initial state of the model. If requested, we can add a sensitivity study for errors/deviations in the initial parameters for 1 of the days.

- 10286,9: "What precluded closing the energy balance in certain cases"
→ The EC method will not lead to a closed energy balance. This is a known limitation of EC (ie. Foken, 2008; Foken et al. 2011). Therefore a site specific

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correction has to be applied in order to compare EC fluxes with surface models that have to distribute all available energy. Therefore, we close the energy balance according to the method of Twine et al. (2000 - see original references), which means distributing the residual according to the Bowen ratio (BR). This requires that BR can be calculated from flux data and it also requires the availability of radiation balance measurements in order to determine the total energy input. We have added a reference to Foken et al (2011) in order to give the reader for information about the current state of knowledge on energy balance closure and have added to a sentence to clarify: "Artificial energy balance closure is only possible, when the Bowen ratio can be determined from flux measurements and when data about the available energy is measured."

- 10287, sec 4.2: "I'm not sure equations for RMSD and cross-correlation are necessary, they are pretty standard metrics."
  → They are standard, however we would like to leave them in the work as we do not find, that they distract from the contents.

- 10292,20: "The authors mention that SEWAB has an instantaneous surface temperature solver, why wasn't something like this tested in HYBRID, because it mentions that SEWAB directly reacts to changes in solar radiation, the very thing that is attempting to be corrected by this simple model. I think it would help the reader why a more sophisticated surface temperature scheme is not tested in HYBRID, especially because this case is a 1-d column and computational considerations are not as important, even if it would be infeasible on a larger scale it would be important to quantify the differences within hybrid between this 'simple' method and a more physically realistic 'sophisticated' method."
  → The original Hybrid is in its code structure very different from SEWAB, as it includes more complex treatment of vegetation and the ecosystem. Therefore introducing a solver for surface temperature is not trivial and would have needed substantial reworking of the model code. It was briefly considered, but then dis-

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carded as it is not beyond the scope of our main research, which is exploring the impact of surface conditions / different surface fluxes onto atmospheric circulation. We also think that our paper shows, that such a simple approach is for our future modelling purposes of comparable quality than a SEWAB like formulation of instantaneous surface temperature.

2 Minor Comments

All technical comments (like spelling or grammar) unless mentioned in this response will be addressed in the revised manuscript.

- 10277,1: “define sufficient resolution” → Order 100s m, added to the text.
- 15-17: “ which model are you modifying” → added model name + references
- 22: “consider moving any references to future work [. . .] to[wards] the end of the manuscript.” → We would like to leave this unchanged as we see it as part of the motivation for our work.
- 10278,16-17: additional information about study region is requested → We will modify the text accordingly, in order to give a better impression of the study area. We believe as the 1D model has a very limited fetch, a figure or schematic of the region would add little to the reader. We can add a photo, giving an impression of the environment at the research area.

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3 References

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 10275, 2011.