Hydrol. Earth Syst. Sci. Discuss., 8, C6043–C6045, 2012
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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.

Anonymous Referee #2

Received and published: 7 February 2012

Review: Turbulent flux modeling with a simple 2-layer soil model and extrapolated surface temperature applied at Nam Co Lake basin on the Tibetan Plateau.

Overview: The authors present and test a simple update to the surface model of the original Hybrid model. The new model uses two-soil layers with an extrapolated surface temperature to improve the calculation of turbulent fluxes. The application of the new model improves issues with a delay in the diurnal flux cycles when compared to the original model. In general, I recommend that the manuscript is accepted for publication after several issues are addressed by the authors.

Major Comments: 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

P10279, line 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 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?

P10280, 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?

P10281, line 9: Please add equations for LE. If it is derived in a more complex manner, compared to sensible heat flux, the reader should be presented the equation set.

P10285, line 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.

P10286, line 9: The manuscript states that EC fluxes were closed with a constant Bowen Ratio assumption, “when possible”, what preclude d closing the energy balance in certain cases?

P10287, Section 4.2: I’m not sure equations for RMSD and cross-correlation are necessary, they are pretty standard metrics.

P10292, line 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.

Minor Comments:

P10276, line 24: define LES? P10277, line 1: define sufficiently high resolution? P10277, line 15-17: it is not clear which model you are modifying? Is there a name, citation to the original model? P10277, line 22: it seems a bit odd to be introducing future work in the introduction section, consider moving any references to future work to a more relevant section towards the end of the manuscript. P10278, line 16-17: this sentence reads a bit odd, “a small lake next to Nam Co lake”, would it be possible to provide a schematic of the study regions and their relationship to other features and each other? P10281, line 27: spelling error “onthe” P10285, line 4: spelling error “rage”

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