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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.

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The authors would like to thank reviewer No. 1 for the thorough and constructive review. We believe that his comments improve the manuscript.

Please find below a detailed discussion of the comments.

1 Specific Comments

- 10278: "The description of the location of the sites with respect to the lakes is not very clear. Site 1 is close to a small lake, which is close to the Nam Co Lake: what direction (east, west ...) from site 1 are both lakes? What is the distance between lake1 and lake2? Is the lake-land breeze originating from Nam-Co Lake or from the small lake? Those questions are not important for the outcome of the study, but I guess it would be nice for the reader to get a better impression of the sites."
  → As noted by the reviewer, it adds little to the scientific contents. However, we will modify the site description in order to give a better overview over the study area. Tying in with a comment by reviewer 2, we propose to add an overview figure indicating the sites and their orientation towards the lake. Proposed new text: "Site 1, referred to and operated by UBT, is an eddy-covariance setup on the south shore of a small lake that itself is situated approximately 500 m south of Nam Co lake. UBT has a fairly constant soil moisture below circa 60 cm depth due to the influence of ground water. Additionally, the atmospheric measurements are influenced by a land-lake breeze that originates from Nam Co Lake."

- 10279: "I think that more days per weather conditions would be reasonable as replicates; From 10th July on UBT was under lake-land breeze effect; what about the period before? At ITP there was no lake-land breeze, at least here it could be possible to find more days with similar weather conditions that could be used as replicates; If the authors have thought about that and neglected that in favor of choosing just 4 single days, please state why replicates might not be of further help in your case"
  → We used data from a field campaign from June 25 to Aug. 08, 2009. The data were selected according to the data quality of turbulence data (Foken et al, 2004) and the wind direction (on- and off-shore wind). Finally we selected four days

with high data quality over the whole day encompassing different weather situation. Due to the full data access the selection could be better applied on the UBT station. On the other hand the ITP station is installed above degraded meadows with a high gravel partition. Further details about data availability, additional measurements and soil structure are given in Biermann et al. (2009). We have added an explanatory sentence to the beginning of the forcing data section (now Section 2.2): "The data used in the modelling study was selected according to the data quality of turbulence data Foken et al. (2004) and the wind direction. Finally, we selected four days with high data quality over the whole day encompassing different weather situation."

- 10279,1: "please explain the sentence "due to the generally drier conditions at ITP, surface temperature frequently drops below 0C in the early morning hours."
  → ITP contains significantly less water and has therefore a lower heat capacity, it is also not that much influenced by the lake which dampens the cooling at UBT. An explanatory sentence about the lake damping the temperature cycle will be added to the text: "Due to the generally drier conditions, reducing soil total heat capacity and the smaller influence of the lake . . . "

- 10279,5: "'less than 20 cm': was there only one temperature sensor at ITP? If not, how many and at what depths? 'Less than' means closer to the surface or deeper?"
  → There were sensors installed at 10, 20, 40, 80, 160 cm depths. Unfortunately, the 10 cm sensor was not in a working state during the period we have EC data. "less than 20" is clarified in the text. Proposed: "At ITP no soil temperatures were available at depths above 20 cm, with data available from 20, 40, 80 and 160 cm below ground."

- 10279,20:"from two sites: THE two sites or other two sites? "
  → these two sites (clarified in text). Proposed:"The model is forced with mea-

sured atmospheric data from UBT (Fig.) and ITP(Fig.) providing"

- 10279,26: " On Aug 06 precipitation was measured, but only for UBT, not for ITP; Is precipitation different between the two sites even though they are in close proximity? Or is the precipitation measurement different, i.e. there was actually rainfall at ITP, which was attributed to the next day, for instance in a rain sampler? Is it valid to interpolate the daily precipitation sums at ITP according to UBT when the measured rainfalls refer to differing time periods? "
  → At ITP daily rain sums were recorded each morning. However, due to a recording error, there is no data at ITP for the measurement on the morning of 7. August. While small scale variations in the occurrence of precipitation are generally possible, the MODIS picture from 23:45 h BST shows a strong weather system over the whole Nam Co region, so that rain will have occurred at both stations. A preliminary analysis of the two datasets found comparable precipitation at the two sites so that the approach of downscaling precipitation is valid. We have modified the sentence to: " However, there was also rain recorded at UBT from about 22:00 h BST on 6 August 2009, while no precipitation data was available at ITP" and modify the caption of Figure 3.

- 10280,9: "Is ATHAM the atmosphere model, where Hybrid should be coupled to in the future study? This information would be nice in the introduction."
  → Yes, we added the information in the introduction in the revised manuscript. Proposed:" In our future studies, the same surface-model version will also be coupled to the spatially and temporally high resolution atmospheric model ATHAM (Active Tracer High-resolution Atmospheric Model, Oberhuber et al,1998; Herzog et al, 1998) . . . "

- 10281, 9: "Please add two equation for LE [. . .] Were the transfer coefficients already modified in the original Hybrid?" → equations will be added to revised manuscript. We did not change the formulation of the transfer coefficients be-

tween model versions of Hybrid. Proposed text: "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):

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

$$TR = \frac{\rho \Delta q_a}{r_c + r_a}, \tag{2}$$

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."

- 10281, 17:"'lower layer with 4m thickness': same setup applied for modified Hybrid model?"
  → Yes, this is built into Hybrid as a storage for a seasonal cycle and we did not change it. We clarify the sentence accordingly. Proposed: "A "thin" upper layer of 10 cm thickness follows the daily cycle of surface temperatures, whereas a lower layer with 4 m thickness acts as the memory for the annual cycle in both model versions."

- 10282,15: "'for both layers' means upper layer with 10 cm and lower layer with 4 m? (which should be clear after 281-17 was modified)" → Yes

- 10282,19: "'no transfer of heat through lower boundary of the model, so that Tbase,2 is constant and equal to annual mean temperature': Is the seasonal cycle of soil temperature still prominent in 4m depth? If yes, I think it would be more realistic to assume a seasonal mean temperature for Tbase,2 instead of

an annual mean temperature. If not, please add a reference to support your assumption; How sensitive is the model on Tbase,2? What effect would have a for instance 10% or 20% variation of Tbase,2 on the simulation results? If Hybrid is sensitive on a variation of Tbase,2 this reveals uncertainty in the simulations and should be added to Fig. 4"
  → The reviewer is correct to state, that some variation of $T_{base,2}$ is expected, even though it should be rather small (i.e. Hillel, 1998). We would expect a 10-20% variation of $T_{base,2}$ to have a rather small impact on fluxes as a one degree increase in $T_{base,2}$ would ie. for Aug 6 at UBT change T_2 by about half a degree, but would have little impact on the formulation in the upper layer $T_{base,1}$ and T_1 on which fluxes are most sensitive on. Using a seasonally changing $T_{base,2}$ is a good idea and will be included for future research and added to the revised manuscript. Proposed: "There is assumed to be no transfer of heat through the lower boundary of the model i.e. T_{base_2} is constant and equal to annual mean temperature of 0 ^{o}C (You et al, 2006, recited from Keil et al.,2010). We are aware of this being a simplification. However, the annual temperature cycle at 4 m is expected to be small and the rate of change as well as the diurnal temperature cycle is too small to have an impact on the day scale. For future research the seasonal mean temperature could be used in order to remove this potential source of error."

- 10282,19: "What is the mean annual temperature at the stations"
  → unfortunately, we cannot calculate a mean annual temperature from our data, as we don't have a full annual record. Keil et al. (2010) cite two Chinese language publications with $< 0\,^o$C for the Nam Co area (Zu et al.; 2004) and 0^{o}C for Nam Co station /ITP (You et al.; 2006). See proposed text above.

- 10283,5: "for a better understanding of the procedure ('by integrating Eq. (3) with Eq. (2) and solving for a2') it would be good to mention that zU = 0 and zL = d2 (at least this is what I assume)"
  → added. Proposed: " ... by integrating Eq. (x) with Eq. (y) from $z_L = 0$ to

$z_U = d_2$ and solving for a_2, thus deriving . . . "

- 10284,3:" 'was modified' ": the exact modification and the difference to the original Hybrid is not very clear; Please add the equation for F(z) as used in the modified"
  → equation 9 will be modified in the revise manuscript:

$$\frac{\partial T}{\partial t} = D\frac{\partial^2 T}{\partial z^2} \approx D\frac{T(z_1 + \Delta z) - 2T(z_1) + T(z_1 - \Delta z)}{2\Delta z} \tag{3}$$

  Heat transfer is as so far moisture dependent as c_p and heat conductivity are dependent on soil water content. In addition to this, the transport of heat by water entering and leaving soil layers is considered.

- 10284,10: "In my opinion it would be clearer to merge 2.1 and 3.3 in one paragraph describing all observations (as there is no 2.2 in the text this needs to be changed anyway) and then clarify the exact procedure of initialisation, which is not very clear to me, see following points"
  → In an early draft of the manuscript, we had all information in a single paragraph. We changed it to the present state as it was difficult to introduce all information regarding the forcing data and the temperature initialisation in a way that one of the two parts did not look out of place. About the numbering: We will change this.

- 10284,18: "As Tbase,1 is taken from measurements, why is calculation of Tbase,1 from Tbase,2 described earlier? Please clarify the exact procedure used in this study and whether there are different options how to initialize the soil profile (e.g. dependent on availability of observations)."
  → $T_{base,1}$ was taken from measurements when available (UBT) and was estimated when not (ITP). The calculation of $T_{base,1}$ from $T_{base,2}$ is the equation is needed for the initial estimation of parameter a_2. We will revise the text in order to make it more clear to the reader. See also our response to sec 3.2.1 and 3.3 for revised text.

- sec 3.2.1. 3.3: "the authors describe a way to calculate surface temperature via extrapolation (based on Tbase,2 and heat content of the soil), at 3.3 they state that temperature measurements were taken for initialization (both heat content and surface temperature) and later they discuss the theoretical parameter space of the initial surface temperature when, as I understood, surface temperature is extrapolated from Tbase,2 given different temperature profiles in the soil. To me, this is a bit confusing "
  → We agree that this section can be made clearer. Surface temperature is dependent on $T_{base,1}$ and T_1 and less on $T_{base,2}$. Proposed changes to text in sec 3.2.1: "As $T_{\mathrm{base_1}}$ is a parameter of both Eqs. (7+9) and $a_{1,2}$ are of crucial importance to the initialisation of $E_{1,2}$, when assigning initial conditions (see discussion in sec. 3.3)." In sec 3.3 our description of the initialisation is now: " We initialised E_2 by setting $T_{\mathrm{base_1}}$ to the measured 10 cm temperature and then subsequently fitted the temperature curve for the first model layer by minimising the squared mean error with regard to measured soil temperatures. Due to the lacking 10 cm temperature at ITP, this temperature had to be estimated from the 20 cm measurement and T_0 was approximated in order to estimate the initial E_1."

- 10285: "SEWAB has been calibrated for the sites; who did the calibration (the authors?) and what parameters were optimized? (general description is sufficient)"
  → SEWAB was set up by one of the authors (W. Babel) for use at Nam Co lake, by using realistic/ measured parameters as displayed in Tab. 1. A publication discussing a site specific calibration and prarmeterisations of SEWAB is in preparation. We replace "calibrated" with "configured".

- 10286,3: " 'especially true for lake land breeze . . . therefore SEWAB and Hybrid fluxes are comparatively larger than measured ones': the periods with occurring lake-land breeze were actually excluded from the comparison between modelled and observed fluxes, i.e. I don't know whether this statement is right"
  → we did eliminate periods where the EC system had no fetch from the land

surface, however there is still going to be some lateral transport. Additionally other reasons for the lack of closure are mentioned before. We propose to replace “This is especially true” with “Additionally, a significant proportion of fluxes is transported . . . ”

- 10286,18: ” ’as the footprint of the EC system and the forcing data ’ Maybe better: ’. . . of the EC system and thus also of the forcing data for the models“
  → agreed

- 10286,26: ” ’for completeness TOGA-COARE fluxes’: I don’t really see the benefit of including TOGA-COARE (and HM) simulations for this study, actually Fig. 5 is a bit overloaded and would be clearer without the TOGA-COARE and HM curves“
  → We would like to keep a reference to TOGA-COARE and possibly HM in order to show for future work that fluxes here are also within reasonable limits. We will take the apparent information overloading of Fig. 5 into account, when redoing the figure.

- 10287: ”Pleas add a comment on uncertainty in initialization relative to uncertainty in the climate model Hybrid will be coupled to at the end of results and discussion or in conclusions“
  → We are not completely sure, what exactly is entailed by relationship between the coupled atmospheric model and the surface initialisation. After careful consideration, we find it difficult to add such a comment in a paper about the surface model. We would find it more appropriate to add this discussion or conclusion to a paper dealing with coupled simulations. In such a paper we can look at the sensitivities of atmospheric circulation on changes of the surface parameters and then derive information about uncertainties.

- 10287: 11: ” ’periods without ECcorr were excluded’: please refer to Fig. 4 and 5 in order to show an overview of the time periods with/without EB correction;”

→ will be done. Proposed: “Periods when no energy balance corrected EC measurements were available (see Fig. 5+6 for details) were excluded from the calculation of the statistical measures.”

- 10287,15: “’when the formulation was included’: was there no formulation for surface temperature before? Suggestion: ’when a new algorithm for the surface temperature was implemented’ ”
  → suggestion appreciated

- 10287,16: “Where can I see the time shift in the original Hybrid simulations?”
  → see response to comment on Figures 5+6 (RC 10307).

- 10288,6,11: ”’compared to the original Hybrid’: again: where can I see that? ’while the original Hybrid showe’ where can I see that? “
  → see response to comment on Figures 5+6.

- 10288,10,18,19: ” ’match closely’: true for QE, even though there seem to be a time lag; for QH however, the simulated fluxes by Hybrid are almost twice the magnitude of the ECcorr fluxes [. . .] ’this starts to diverge’: what? The curves or the fact that Hybrid is closer to SEWAB than to EC? The wording is a bit irritating, please rephrase“
  → will be rephrased and clarified in an updated version of the manuscript. 11: in this case we mean by dynamic the general shape of the curve and not the magnitude (which is close to SEWAB but much greater than EC. Proposed: ”On 5 August the turbulent flux dynamics, but not the magnitude of the fluxes, match the EC measurements closely (Fig. 6), while the original Hybrid showed a strong delay in the flux response as the soil remained frozen during the morning. While the magnitude of the latent heat flux is close to EC measurements, Q_{H} produced by Hybrid are of a similar magnitude as Q_{H} from SEWAB. These are considerably larger than the fluxes measured by EC and corrected for energy balance closure.“

18,19: Unclear formulation was revised to: " Q_H in contrast shows similar diurnal dynamics as $Q_{H_{EC,EBC}}$, but with its magnitude between the sensible heat flux derived by SEWAB and $Q_{H_{EC,EBC}}$. Around 18:00 h the Q_H-fluxes from the different methods become more similar."

- 10289,10: "'surface temperature is purely diagnostic and dependent on [mean]T1': this dependency gets not very clear from the model description yet, as the equation for F(z) is only given for the original Hybrid"
  → should be clearer from the improved model description resulting from other comments of reviewer 1.

- 10290, 19: "Is there no large temperature gradient between the surface and the air at measurement height and are there no strong winds at UBT? Why not?"
  → The reviewer is correct to state that environmental conditions are similar at both sites. After reviewing the data and our statement in the text, we come to the conclusion that for ITP additionally a frozen soil (in the model) delays the reaction of the surface. The strong wind from c. 5 am and the rather large temperature gradient lead to a stong negative heat flux. At UBT the soil remained above 0^oC, so that there was a less negative heat flux. The manuscript will be modified accordingly. Proposed: "The large negative and potentially unreasonable night-time Q_H-fluxes that are modelled for ITP on 06 August are owed to a frozen soil and strong surface winds that lead to an overestimation of the temperature gradient, delayed reaction of the surface model and resulted in an potential underestimation of modelled surface temperatures and thus surface fluxes."

- 10293, 10: " 'realistically estimate turbulent surface fluxes': turbulent fluxes are not really simulated by the soil model; the soil model provides a better surface temperature, which then results in improved turbulent fluxes"
  → This is true, we will clarify this in the revised draft: "simulate skin temperatures and thus to generate more realistic surface fluxes."

- 10306: "Please add the source for the soil characteristics; how sensitive is Hybrid on those input parameters? It would be good to know whether the uncertainty in the simulations resulting from soil parameterization is comparable to the uncertainty resulting from the initialization of heat content etc. for the modified Hybrid; Vegetation height 0.15 vs. 0.07 and lai 0.6 vs. 0.9 are different for both sites (ITP vs. UBT): is there an explanation why the grass grows higher but with lower lai at ITP? Values for lai are equal to vegetated fraction: is this a coincidence or was vegetated fraction derived from lai? Albedo for bare soil is usually different to albedo from grass, i.e. as vegetation fraction differs between the two sites there should also be a small difference in the albedo values"
  → The surface data was collected by our group or was estimated in the field. Unfortunately, there is no direct reference. We can add the technical report as reference (Biermann et al., 2009). The albedo should be different at both sites due to the difference in vegetation fraction. However, the soil at ITP was measured with an albedo of 0.20 compared to an soil albedo of 0.11 at UBT. Combined surface albedo at both sites works out at 0.20. Vegetation cover and height were determined in the field. There was no independent estimate of LAI. The LAI in Table 1 refers to the total LAI of the site and not to the LAI of the vegetated fraction. Instead LAI was set to 1 m^2 m^{-2} for the vegetated fraction taken from the Damxung site (Hu et al., 2009), which is located 30 km away on the other side of the Nyenchentanghla mountains, that has a similar ecology to ITP.

  We found a negligible sensitivity of the model small changes in the soil parameters of both models and a limited sensitivity ($< 15\%$) on changes in LAI in preliminary studies. We think that the main focus on the paper should be on the method and on the temperature, so that we would like to not overload the article with sensitivities of the surface parameters. We propose to change caption to: "Description of the two sites (UBT and ITP) near Nam Co lake and the parameters used the model setup (Biermann et al., 2009)" and add: "estimated from Hu et al., (2009)" to the LAI line in Tab. 1.

- 10306: I like figure 4. But where is the dependency of Tbase,1 on [mean]T2 in the equations? What I would like to see in addition is the dependency of T0 on Tbase,2.
  → in the current state of the model there is no direct dependency of T_0 on $T_{base,2}$. This dependency only arises as a consequence of $T_{base,2}$ influence on $T_{base,1}$. We will amend the manuscript text in sec. 3.3 with a discussion of Fig. 4a and will add a references to equation (7) (dependency of T_2 and $T_base, 1$) into the caption of said figure and manuscript text. Proposed: " Table 2 shows the initial temperatures for each day. From the span of layer temperatures $\bar{T_1}$ and $\bar{T_2}$, the theoretical parameter space of T_0 for a constant T_{base_2} (Fig. 4) can be derived. While Fig. 4a + b show the individual dependence of temperature variables on each other as expressed in the respective Eqs. (7 + 9), Fig. 4c shows the combined effect of parameter variation. A random combination of the initial temperatures given in Table 2 would yield T_0 in the rage of -10 to 30 ^{o}C. . . "

- 10307-8: "the different red and blue colors are good to distinguish at the monitor but not when printed; you could use the same color for the same fluxes in both graphs as was already done for LSEWAB and then use colors that are better to distinguish"
  → This will be fixed. Our printer produced a good contrast between the shades of color, so that we believed that this was not an issue.

- 10307, Fig.5: "As the authors often refer to the simulation with the original Hybrid, I would recommend adding the curve here, or, if the graph gets too confusing, adding another graph for the original Hybrid simulations; ALL refers to the complete available time series: why are there gaps? Do AllEC -curves represent the time periods where EC measurements could not clearly be assigned to land or water? 'All' is also used in Table3, which might be confusing, maybe it's better to use different terms"
  → Figure will be changed according to the reviewer's suggestion. 'All' includes all

EC measurements (there are some instances though, where no measurements were available, hence the gaps) meaning fluxes originating from land, lake and mixed fluxes that could not be attributed to either regime. We will make this clearer to the reader. We will see whether is is feasable to add another line with the original Hybrid results to Fig. 5+6. In case that this will render the figures too overloaded we propose to add additional figures of the original Hybrid into an appendix.

2 Technical Comments

All technical comments (like spelling or grammar) unless mentioned in this response will be addressed in the revised manuscript. The manuscript contains a space between units typeset at `m\,s^{-1}` as required by the latex guide, however the HESSD font makes the space appear to be missing.

- 10277,15: "two layer flux algorithm": two soil layers or two atmosphere layers?
  → "In this paper we present results of a rather simple flux algorithm based on a modified two-layer soil model"

- 10277,18: "upper model layer?: soil or atmosphere? → changed to "the model's upper soil layer"

- 10278,18: "influence of a water table: I'm not sure whether you can say it that way; 60cm is the water table due to the influence of the ground water;" → changed to "due to ground water influence". The ground water table is deeper than 60cm, but we reach the zone where capillary rise of water plays a role.

- 10278,22: "which lake" → clarified

- 10282,2: suggestion: ”In order to improve the delay in diurnal flux evolution and the weak responsiveness . . . , new simulation approaches for surface temperature and heat diffusion were introduced in Hybrid.“ → changed
- 10300: ”I guess “heat capacity” is soil heat capacity“ → it is dry soil heat capacity opposed to the heat capacity in equation 3, which includes contribution from water. Changed to ”dry soil heat capacity“
- 10300: ”decrease line spacing“ → this was typeset by the Copernicus office, has been changed with modification of Tab 2.
- 10304: ”Precipitation at ITP was not measured in 30 min intervals“ → Explanation added: ”Precipitation at ITP was measured daily and for the purpose of this study distributed to 30 minute intervals according to the recorded rain fall at UBT.“

3 References

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