

Interactive comment on “Snow distribution over the Namco lake area of the Tibetan Plateau” by M. Li et al.

M. Li et al.

Received and published: 24 May 2009

Reply to Anonymous Referee 4

Interactive comment on 8220; Snow distribution over the Namco lake area of the Tibetan Plateau 8221; by M. Li et al. Anonymous Referee 4 Received and published: 19 May 2009 Review of: Snow distribution over the Namco lake area of the Tibetan Plateau By Li et al. Li et al. state that they investigate the impact of the snow distribution on WRF simulations over the Namco lake area of the Tibetan Plateau. As the authors acknowledge, the energy and water exchange between the land surface and atmosphere on the Tibetan Plateau has an important effect on the Asian monsoon. Since snow changes the surface albedo significantly, the presence of snow will have an important effect on the surface energy balance and will, therefore, impact the atmospheric circulation. Although this topic is of great interest for HESS readers, the text in

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



the manuscript is somewhat confusion and lacks a clear objective. I recommend, therefore, a major revision before it is published in HESS. Major issues: In the title, abstract and introduction the authors indicate that they study the 8220;lake-effect snow8221; within the WRF meso-scale climate model. After further reading, authors obtain a better simulation of the WRF snow distribution by using satellite based skin temperature. Therefore, I think that authors are actually improving the skin temperature simulations, which results in a better simulation of the snow distribution.

Re: Yes, we improved the skin temperature simulations, which results in a better simulation of the snow distribution.

Also, the term 8220;lake-effect snow8221; is somewhat confusing. Are the authors referring to the snow on the lake (in Fig. 6 there is no snow on Namco) or do they also refer to snow on the land surface around the lake? With their control simulation the authors show that there is a large deviation between the modeled and satellite observed skin temperature over the Namco lake. By inserting the satellite observed skin temperature an attempt is made to improve the WRF simulations. Therefore, I think that 8220;lake temperature effect8221; (or something along those lines) would be a better term.

Re: we use 8216;lake temperature effect8217; instead of 8216;lake 8211;effect snow8217; and we have already revised it in the revised paper.

Further, I notice that WRF model is run over a relatively short period from 6 October through 10 (or 8??) October 2005.

Re: It should be from 6 October through 10 October 2005.

Since the authors mention that the snow (temperature) effect on the lake have an important effect on the development of the Asian monsoon, I think the manuscript would gain more weight if the authors would run the WRF model (if possible) over a longer period; preferably up to the Monsoon of 2006. Then, they can truly evaluate whether

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



the insertion of the skin temperature has a positive effect on the model performance (or they should clearly formulate an objective, which makes such analysis unnecessary).

Thank you for your good suggestion. We will run the WRF model over a longer period in the near future.

Other issues/suggestions: - Model description: The description of the WRF model and Noah LSM is quite short and limited. I am sure authors can provide more information; specifically on the governing equations.

Re: we have already revised it in the revised paper. - Remote sensing skin temperature: More information is required on the satellite based skin temperature: Which sensors? Overpass time? Number of satellite images used?

Re: FY-2C was launched on 19 October 2004 . FY-2C had an onboard visible and infrared spin scan radiometer (VISSR), which senses four infrared channels: (IR1, 10.3-11.3 μm ; IR2, 11.5-12.5 μm ; IR3, 6.3-7.6 μm ; IR4, 3.5-4.0 μm) and a visible channel (0.55-0.90 μm). In this study, we use 11 micrometer brightness temperature (T11) as surface temperature of Namco Lake (T_{sfc}) during clear sky condition

- Sensitive simulation: The authors presented the sensitive simulation as the WRF run, in which the satellite based skin temperature is inserted. The term 8220;sensitive8221; seems to be misused here. In my opinion, the sensitivity of the model would have been analyzed if the authors systematically change the skin temperature several times and evaluate the impact of this change on the model output. Instead the authors use just one satellite based skin temperature and insert it into the WRF model. This process looks more like data assimilation. Also, it is not clear to me how the skin temperature is inserted. Is it used as a replacement for the initial conditions? In other words: this sensitive experiment should be renamed and better described.

Re: Yes, it is used as a replacement for the initial condition. We use 8216;revised run8217; instead of 8216;the sensitive experiment8217; in the revised paper.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



- Results: Section 4.1: The analysis of the near-surface meteorology is limited to a description of the results only. More in-depth analysis is needed as to why, for example: - modeled air temperature is lower than the observed during midday; - wind speed and direction simulations represent the measurements less well than for the temperature; Also, why not plot the air temperature, wind speed and wind direction for the control run in Fig. 3? In that case the reader can see whether the insertion of the skin temperature improves the model performance.

Re: The heating process like the sensible heat flux and latent heat flux and underlying condition in model like frozen soil and ground heat and so on will be analyzed in the next study.

Section 4.2 - 4.4: It is interesting to see that the surface temperature of the lake fixed at 300K has such a strong effect on the snow production by model and the simulation atmospheric circulation. I am missing, however, a discussion on how this shortcoming effects the WRF simulations on the long term. Also, the difference in snow depth and distribution between the two model runs is striking, but it is unclear what the actual situation was. Perhaps the authors could validate these snow distributions using snow products from MODIS.

Re: We will validate these snow distributions using snow products from MODIS in the next study.

Minor comments: It is unclear over which period the model has been run. In some parts of the manuscript the period 6 October to 10 October is mentioned in others 6 October to 8 October. Further, there are several typos throughout the manuscript; some are listed below: P843L10: 8216;suggested8217; -> suggest

Re: we have already revised it in the revised paper. P844L24: 8216;influence8217; -> influences

Re: we have already revised it in the revised paper. P845L14: 8216;occur8217; ->

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



occurs

Re: we have already revised it in the revised paper. P845L20: 8216;of8217; -> for

Re: we have already revised it in the revised paper. Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 843, 2009.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 843, 2009.

HESSD

6, S635–S639, 2009

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

