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Hydrology and Earth System Sciences Discussions

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

Anonymous Referee #4

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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 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 “lake-effect snow” 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.

Also, the term “lake-effect snow” 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 “lake temperature effect” (or something along those lines) would be a better term.

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

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.

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- Remote sensing skin temperature: More information is required on the satellite based skin temperature: Which sensors? Overpass time? Number of satellite images used?

- Sensitive simulation: The authors presented the sensitive simulation as the WRF run, in which the satellite based skin temperature is inserted. The term “sensitive” 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.

- 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.

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.

Minor comments:

It is unclear over which period the model has been run. In some parts of the manuscript

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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: ‘suggested’ -> suggest

P844L24: ‘influence’ -> influences

P845L14: ‘occur’ -> occurs

P845L20: ‘of’ -> for

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