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Hydrology and Earth System Sciences Discussions

Interactive comment on "WRF simulation of a precipitation event over the Tibetan Plateau, China – an assessment using remote sensing and ground observations" *by* F. Maussion et al.

Anonymous Referee #1

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General comments:

The manuscript is to report an assessment of using WRF model to reproduce precipitation over the Tibetan Plateau, China. The topic is very interesting. However, the approach method and the result of this study are somewhat not quite convincing. Therefore, it is not recommended for publication in its current form.

Specific comments:

1. Usually, the model is set up to run continuously for one case study. For example, if an event lasted for 3 days, one would carry out a 72-h simulation. This way the

model performance can be fairly evaluated, and the evolution and the development of the rainfall system can be well examined. However, the simulation in this study is separated for 7 runs and only the precipitation between 12-36 h of each run is used for verification. I don't understand the purpose of this kind of modeling setup. If the WRF simulations are to provide rainfall or snowfall information over the mountains, it will be more desirable to do verification of 24-h accumulated precipitation. For example, calculating the scores by comparing the data in Fig. 2.

2. The WRF is run with 3 nested domains of 30, 10, and 2 km resolutions. This is quite a high resolution simulation. The model performance should be verified in somehow a mesoscale way. But, instead, the precipitation is verified against TRMM and MODIS data using a 7-d accumulated rain or snow. This is really disappointing. Why don't the authors verify 24-h accumulated precipitation forecast against observations?

3. The comparison with TRMM data is only to tell the performance of WRF30. If the results are not bad, it would be more interesting to discuss the high-resolution simulations of the event in WRF10 and WRF2. But, the authors chose not to examine any other fields except for precipitation. If the rainfall is comparable with the TRMM observation, what will be the purpose of the simulation if the authors did not look further? We could just use TRMM data to provide the precipitation information over the Tibetan. Of course it would be another story if the model setup is designed in a real-time forecast mode. In that case, one can say that WRF can be used for forecasting purpose. However, this is not the intention of the study.

4. The same situation aforementioned applies to the comparison between WRF10/WRF2 and MODIS snow data. I cannot see much value from the comparison. If I have MODIS data, I will trust the observation more than the model simulation. Furthermore, the verification is only made for the 2mm/7d and 7mm/7d thresholds. The authors did not show how much snow was estimated in the MODIS data. But, I guess 2 or 7mm of snow is very small for a 7-d accumulation. The scores are usually higher for smaller thresholds. The result presented here does not necessarily guarantee a good

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model performance.

5. If the authors intent to evaluate the model performance over the Tibetan, more case studies are needed. It can not be over emphasized by just one case. If possible, a statistical evaluation of one-month simulations would be better. For example, how does the model perform if there is no precipitation?

6. The conclusion about different settings of model physics is not convincing. It is made only based from a simple precipitation score. More examinations are needed in order to make a robust conclusion about the performance of different physics.

7. Overall, there is not much scientific contribution, and not much we can learn for the forecast purpose, either, from this paper.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 3551, 2010.