Hydrol. Earth Syst. Sci. Discuss., 7, C1762–C1766, 2010
www.hydrol-earth-syst-sci-discuss.net/7/C1762/2010/

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.

F. Maussion et al.

fabien.maussion@tu-berlin.de

Received and published: 11 August 2010

Authors' response to Anonymous Referee 2 reviewer comment (C1739, 09 Aug 2010)

General reply

We are thankful to the Referee 2 for reviewing our manuscript.

Some questions raised in this comment are similar to the first referee's remarks, and we kindly ask the reader to refer to our answer 1 for a more comprehensive description of the study's objectives. We understand from the two reviews that the presentation of

the modelling approach and its results has to be clarified. Also, expanding the analysis to a longer time period in the discussions will reinforce the general conclusions that we learn from this specific precipitation event. We wish to include these changes in a revised manuscript.

Reply to specific comments:

RC: The authors intent to evaluate the performance of WRF model over Tibetan Plateau, especially for precipitation. But they only use one special case in the manuscript. The general conclusion can not be drawn by one case, especially over Tibetan Plateau with complex topography. It is suggested to statistically evaluate the long-term simulations, such as one year or at least three months. And it should include some heavy precipitation events, and some light precipitation cases.

AR: The conclusion's statistical relevance is affected by the lack of observational data (for which there is few we can do) and by the short period of time considered. Conducting seasonal to yearly simulations would move away the study too far from the initial objective, which is to concentrate on this cyclone over the Bay of Bengal causing strong rain- and snowfall over the Plateau. However, it is true that the conclusions would gain in relevance when placed in a brighter context. We therefore propose to compare the output from WRF over the month of October with TRMM and stations observations. This would also allow evaluating the model's response when no precipitation is observed, and determining the significance of this specific event in the monthly precipitation rate.

RC: The WRF model is performed with 3 nested domains (30, 10, and 2km). But the authors did not discuss much about the performance of WRF2, only in section 3.3. It should be very interesting to analyze the model performance at such high resolution. And I suggest to compare the time-series and diurnal cycle of precipitation between WRF output and observation.

AR: The performance of WRF2 can be assessed using MODIS (Fig. 4, 5) and the

four available stations' measurements. We saw no robust argument to trust the 2 km resolution more than the 10 km resolution looking at ground observations. Figure 4 shows that the WRF10 results are slightly superior to WRF2 as measured by the HSS. To allow the reader to evaluate the differences both visually and statistically, we will enhance the figure 5 and include the results from the three resolutions over each inner domain (medium and small).

The time-series of TRMM, WRF and station observations are showed in Fig. 6. The temporal resolution of the stations' measurements does not allow a diurnal cycle comparison. Moreover, the event does not show a clear diurnal cycle in both TRMM and WRF outputs (Fig. 6). As stated in comment 1, the daily scores from WRF in reference to TRMM will be evaluated.

RC: I am not so clear about the comparison of snow data between WRF output and MODIS. First, how to define the snow amount and snow cover from WRF output ?

AR: It is right that the information contained in the MODIS dataset is only on snow-extent and does not contain information about snow amounts. It is the purpose of Fig. 4 to present the score of WRF for each threshold in a physically reasonable range (1-50 mm/7 days).

RC: Second, Why the threshold values are so different from the same WRF10 experiment but just over medium and small domain (Figure 4.) ? The authors get the 7mm threshold from the best HSS score of WRF10, so how about the threshold for WRF30 and WRF2 ? I do not think that this threshold is physically reasonable.

AR: WRF10 over the medium domain shows the best concordance with MODIS at the 7 mm threshold (Fig. 4). Moreover, the continuity of the score over the range 5 – 20 mm/7 days is an indicator of a good stability: the threshold of 7 mm has been selected because it represents the best performance, but the results will vary only slightly with a threshold of 10 or 15 mm, which are physically acceptable and can be detected by MODIS sensors.

The results from WRF10 over the smaller domain are to compare with WRF2 over the same area. This comparatively bad scores show that the model is not as efficient over this region of interest that it is considering the whole Plateau. Both WRF10 and WRF2 reach their maximal score for low thresholds, which is indeed too low and should be closer to 7 or 10 mm/ 7 days. The purpose of Fig. 5 is to show that the scores vary strongly between the two thresholds (2 and 7 mm/7 days) regarding to the spatial distribution of snowfall. The north eastern snow limit in the small domain is not caught properly, which lowers the scores. The spatial accuracy in snowfall retrieval that the model has to reach for this test is high, it is not surprising that WRF cannot perform as good as it does in the larger scales.

More generally, the figure 5 will be modified for a better presentation and readability of the results, and extended to the different model resolutions.

RC: As to the influence for model physics, only precipitation and snow scores are used to evaluate the impact of different model physics on WRF simulations. More statistical evaluation should be done.

AR: As the study focuses on precipitation, the statistical tools that are described in the first part of the manuscript are then applied to the different experiments. The reviewer is right that the conclusions will gain in relevance with more tests, but it is not our purpose to realize a comprehensive sensitivity study about the model's physical parameterizations. Our main finding (useful when using the model in a downscaling configuration for hydrological studies) is that there are only small differences between the experiments. We showed in the supplement to *answer 1* that a different reinitialization approach has in fact much more impact on the final result that the model's internal physics. This important conclusion will be added in a revised manuscript.

RC: It is very interesting that the thresholds of snow from HSS max vary largely among different physical schemes, especially for WRF2 experiments. Are there any physical reasons to explain it ?

AR: It is true that the LS1 experiment (change of the land-surface parameterization scheme) shows a larger difference than the other experiments in the comparison with MODIS. Interestingly, this scheme shows less influence on the other tests based on liquid precipitation. The reasons for this will be analyzed further on in the revised manuscript.

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