Author's response and point-by-point response to the re-reviews

Title: Modelling of snow processes in catchment hydrology by means of downscaled WRF meteorological data fields (discussion paper)

Title (new):   Effect of meteorological forcing and snow model complexity on hydrological simulations in the Sieber catchment (Harz Mountains, Germany)

Author(s):   K. Förster et al.

MS No.:   hess-2014-76

MS Type: Research Article

Iteration: Minor Revision

We would like to thank two anonymous referees and Dr. Bettina Schaefli for their effort to re-review and enhance our manuscript. We appreciate the proposed suggestions and corrections and we believe that these helped us to further improve the manuscript. This document includes the reviewer's comments (blue) and our detailed answers (black). The changes in the manuscript are highlighted in text.

Reply to Anonymous Referee #2

General comments:

I appreciate the changes proposed by the authors. In particular the comparison with the temperature-index method driven by observed temperature and precipitation. Now, the advantages (and limitation) of using LAM data for hydrological modeling in the Sieber catchment are more clear. The study is interesting if one is considering the possibility of using LAM data (partially or totally) for forcing snow models. In particular, the adoption of four snow models helps in investigating the actual usefulness of WRF data and makes the results more robust. The results demonstrate that for this catchment it is possible to get acceptable hydrologic simulations using LAM inputs and avoiding observations. An interesting point is that dynamic downscaling in complex topography is expected to be improved in the future, thanks to the numerous researches directed to this target. Thus, in perspective the use of LAM fields without observational dataset may be regarded with higher interest. I think that this concept should be stressed in the introduction. Your work gives an indication in this direction. However, you have still used observed precipitation for the calibration. This helps the performance since simulated precipitation did not fit well observations. In your work, analysis and results are still partially dependent from observations. I think this limitation should be highlighted, at least in the conclusions.

We thank anonymous reviewer #2 for her/his detailed comments on the manuscript. The comments and suggestions helped us to improve the manuscript. We have followed your suggestion and added a section that describes the value of LAM fields with respect to independence of local observations and the ongoing scientific progress in atmospheric modelling. Thank you for pointing us in this direction. Moreover, we have added one sentence that highlights our study's dependence on observations.

Then, Some points need further clarifications.

Introduction:

In my opinion, this question is not clear: "to what extend does LAM data enhance model performance?"
First: the right expression in English should be "to what EXTENT"
Second: You use the word "enhance". In comparison to what? Are you wondering if it is possible to increase model performance using LAM instead of observations (of temperature and precipitation)? Are you discussing the value of LAM data in general, as alternative data source? I think your point is the second one, since in the abstract you stated: "...are better reproduced by application of observed meteorological input data".

We agree that the first question is not as clear as it should be and we adopted your suggestion as described below.

Focusing on your work, I think the right question could be something like:
"Given the possible lack of observed data on several meteorological fields (eg humidity, wind speed, and sometimes even temperature - as the Editor stated in her comment to the first version -) does LAM data represent a worth alternative to observations for modeling snow processes in hydrology? Could LAM downscaled fields help in areas where there are no observations available, for instance providing some meteo fields necessary for the energy-balance?" The answer is: yes, for this case study. But if at least observed temperature and precipitation are available, a simple degree-day method is still able to exceed the performance of complex snow model driven by WRF meteorological fields.

You are right to say that this question is more appropriate in this context. We adopted your suggestion and changed the first question accordingly. As described later, we now also state that the performance of the temperature-index approach is still good when observed data is used.

Model calibration:
Is there a specific reason why you calibrated the hydrologic model manually? 10 parameters would suggest a calibration using a algorithm
or Monte Carlo method. Please put light on the reasons for your choice and explain why you believe that your calibration is satisfactory. All the results depend on it. However, the efficiency seems good enough in my view.

Thank you for this comment. Indeed, a model calibration using algorithms (e.g., Monte Carlo simulations) would have been of great value in order to calibrate the hydrological model. As described in the Hydrological Modelling section, a model setup of the Sieber catchment was already available for our study. The respective parameter set has been derived through manual calibration. As you state, we also found the model performance to be good enough for further investigations. Consequently, we also calibrated the snowmodels manually. However, we will keep your suggestion in mind and we will consider this for future work.

Results and discussion:
Please introduce this Section by stating his structure and content in few lines. I don't like the jump 3 - 3.1 - 3.2 without any explanation on what the reader is going to read.
Please add an additional summary table, such as Tab. 3, with model efficiency for the validation period 2010/2011.

We have added a brief summary about the structure of the Results and Discussions section. Moreover, the results of the validation period are now available (new table #4).

Summary and conclusions:
- I cannot find the answer to the question 1) presented in the introduction.
- Please, state that your study shows even that the temperature-index approach, despite its simplicity, is still able to reach the performance of more complex energy balance model, when it is forced by observation while the others by WRF data. The choice of a hydrologic modeller could be: a) no observations: WRF data and energy balance approach, expecting reasonable results both at point and catchment scale (better than temperature-index driven by WRF). b) Only temperature and precipitation data available: temperature-index using observations or energy balance coupling LAM with observed data. Only observed precipitation (common situation for the Alps, as stated by the Editor): energy balance approach, using WRF downscaled data and observed precipitation.

Yes, the answer was missing. We have added a paragraph to the last section referring to the updated first question. Thank you for this comment. Of course, you also correctly state that the temperature-index approach performs well using observed meteorological observations as input. We describe this good performance of the temperature-index approach on page 22 and line 13 in the revised manuscript.

- P 22 L 4 Please state clearly that your study gives also an indication about where are located the limits of WRF downscaled data for hydrologic applications (in downscaled precipitation). This is an interesting result by itself. Here, give another hint about your previous study using observed precipitation coupled with WRF data (previous version of the manuscript): it was certainly interesting but it did not agree with title and abstract.

We have added one additional paragraph to the last section. We now refer to the previous work that investigates combined meteorological forcing (observed precipitation and all other variables derived by WRF). Thank you.

Reply to Anonymous Referee #3
General comments:
The authors have taken a significant effort and done a good job in revising the MS, which is now majorly improved. I suggest to accept the MS after some minor technical corrections.

Specific Comments:
P4: Line 26: ..quality snow model results. "of" is missing
P5: Line 1: "require" instead of "requires"
P7: Line 14: Please remove one "spatial"

Answer to Anonymous Referee #3
We thank Anonymous Referee #3 for re-reviewing our manuscript. We corrected the manuscript with respect to the mentioned points of criticism. Thank you!