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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Modelling of snow processes in catchment hydrology by means of downscaled WRF meteorological data fields" *by* K. Förster et al.

K. Förster et al.

kristian.foerster@uibk.ac.at

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We would like to thank Anonymous Referee 2 for a detailed review of the manuscript. The comments will help us to improve our manuscript. Please find below our detailed response.

General comments:

> *"The paper deals with the value of WRF downscaled meteorological fields for driving snow modules. Ground measurements usually limit performances of hydrologic model in mountain catchments, where atmospheric*

forcings may vary within restricted horizontal distances due to topographic effects. Several published contributions describe the value of downscaled meteo data for driving energy-balance/temperature-index snowmelt models. Here, the innovative contribution is related to the use of four independent snowmelt models, as it is indicated within the introduction. This should help in making results model-independent, if one is investigating the value of downscaled inputs for modeling snow processes.

Overall, the topic is interesting and meets the requirements of the journal. Methods are clearly explained with synthetic sentences.

The assessment of the manuscript is highly conditioned by the fact that downscaled precipitation is not involved. This invalidates title, abstract and introduction. It even affects results, since it is not possible to draw conclusions about the usefulness of WRF atmospheric forcings for modeling snow accumulation and snowmelt. Your approach simulates well both discharge and melt runoff, but it is still dependent on in-situ data. Results do not provide useful information if one wants to applied WRF outputs for hydrologic purposes, thus avoiding observations. These limitations must be overcome if you are going to maintain the same targets for your paper. Results obtained using downscaled precipitation must be shown in accordance to the title."

We agree that involving modelled precipitation for snowmelt simulations would be more appropriate with respect to title, abstract and introduction. Using simulated precipitation would indeed give an insight into the applicability of downscaled data. Thus, we will revise our manuscript with respect to this issue.

"An idea could be an additional comparative study between the performances you got at the point scale, driving snowmelt models by WRF fields

HESSD

11, C1535–C1541, 2014

(including precipitation, even if the simulation is not good) and the results obtained using in situ measurements (you stated that temperature and precipitation are recorded: these seem the key factors and they are certainly enough for the temperature-index approach and maybe for the model Walter et al. 2005.). This will give an indication about the loss of accuracy due to downscaled forcings when compared to reliable in situ data."

We wish to thank Anonymous Referee 2 for this suggestion. We believe that such a comparative study would be very helpful in order to improve our manuscript. We will take the suggestion into account and we will compare temperature-index simulations for both cases (observed and simulated meteorological input).

"Again, catchment scale simulations should use downscaled precipitation. Here, a comparison could be performed for understanding whether this approach outperforms the results provided by a spatial distribution of observed temperature and precipitation. You spent several sentences on the issue of "area representativeness of point observations" within the introduction. At the end, it is not clear if WRF data help for hydrologic modeling when compared against standard inputs (ie ground-based measurements). Methods for distributing point values (eg kriging) exist. For instance, using the simple degree-day approach, it could be implemented a spatial distribution of temperature with altitude (a constant lapse rate is usually adopted). Then, you can run the degree-day model coupled to PANTA RHEI."

For the catchment scale simulations we will also present results that rely on downscaled precipitation. According to your previous suggestion, which focuses on the point scale simulations, a similar comparative study for the catchment scale will be carried out and presented. Therefore, two configurations of PANTA RHEI / temperature-index input will be taken into account: i) standard input including observed precipitation and

HESSD

11, C1535–C1541, 2014

temperature assuming a constant lapse rate and ii) downscaled temperature and precipitation. This comparison also emphasises that it was not possible to derive precipitation fields with high accuracy at the considered spatial and temporal scales.

> *"It is not clear whether you are discussing the value of the combined use of WRF and ground-data or the value of WRF meteo data alone. In the first case, you should state that your study uses both data sources from the beginning, and the results must be discussed with this focus. Even in this case, additional analysis should be provided. For example, it could be possible to use precipitation and temperature recordings (which seem available at several stations around the catchment) and downscaled radiation, wind speed etc., if such data are not measured."*

We think that your previously mentioned suggestions will also help us in the process to improve our discussion. Since we will show results based on both observed and downscaled precipitation, we will focus on the value of all downscaled meteorological variables, even though precipitation is not as accurate. As explained in our earlier comment (15 May 2014), we suggest using observed precipitation if available.

Specific comments:

> *"If you are not going to use WRF precipitation the title should be changed. This must be claimed also within the abstract. In the title, the sentence "modelling of snow processes in catchment hydrology" seems very general. Actually you are considering only one (and particular) case study. Probably the title should indicate this."*

As explained in our reply to your general comments, we will consider downscaled precipitation in our revised manuscript. Hence, we think that it is not necessary to change

HESSD
11, C1535–C1541, 2014

our title with respect to precipitation. However, we could indicate the study area through providing a subtitle: "A numerical experiment for the Sieber catchment (Harz Mountains, Germany)"

> *"P 4066 Line 5: I do not think this is always true. Please state that the problem of spatial resolution is mainly related to complex topographies of mountain regions."*

Indeed, this statement holds especially in the case of mountainous terrain. We will rephrase our statement accordingly.

> *" 'Study area' section: It is not clear the whole number of stations available around the catchment and what kind of data are provided (only temperature and precipitation?). This is useful since you might run the models using ground observations and see what happens in comparison with WRF inputs. This might be an interesting contribution by your paper."*

As suggested by Anonymous Referee 1, we will add the station network to the map depicted in Fig. 1. We will also indicate the variables measured at each station.

> *" 'Selected winter season' section: I agree with referee 1 that two particular winter seasons" are not enough for drawing conclusions. Please consider the possibility of involving a third hydrologic cycle."*

We are in the process of preparing a third winter season.

> *" 'Snowmelt models' section: please, spend some times in describing what parameters you calibrated, perhaps by inserting a summary table. Are they calibrated using melt rates from the lysimeter?*

HESSD
11, C1535–C1541, 2014

Could be possible to better explain how you designed accumulation and melting for the temperature-index model? Did you consider refreezing? Are you using only one degree-day factor for the entire basin? Did you consider the additional energy input by rain-on-snow?"

We will briefly describe the parameters that have been altered in the calibration process of each snowmelt model. Refreezing and different energy sources were neglected and we provided land use dependent degree-day factors. Calibrations were carried out using lysimeter data. This information will also be added.

"P 4074 line 9: how did you calibrate snowmelt models for catchment scale simulations? What did you calibrate?"

Each snowmelt model was calibrated through altering the parameter most relevant for canopy effects. The description of this approach will be added to the text.

"P 4077 line 20: please remove comma after 'concluded'."

Done.

"P 4081 line 19: the fact that considering snow processes instead of "no snow" you are improving runoff simulations is not a finding. It would have been a problem if it happened the opposite. On the contrary, you should discuss how much you improve the "no snow" simulation and if it justifies the use of a more complex hydrologic model. Anyway, your sentence does not seem scientifically relevant for the goals of your manuscript."

We will delete this sentence.

HESSD
11, C1535–C1541, 2014

"P 4082 line 3: to do what? you are combining data sources. Please explain why and where the presented approach could be applied."

We will rewrite these concluding remarks according to our first comment and the updated simulations. Using observed precipitation in combination with downscaled data could be seen as an alternative for regions where e.g. only a few precipitation observations are available and all other meteorological data are missing.

"Fig. 5: the plot is not very clear due to the high frequency. Please consider to enlarge the x-axis or restrict the temporal window you are showing. Otherwise you may split it into two time frames."

The aspect ratio of the axes aspect ratio will be improved accordingly.

"Fig. 8: why the name "snowmelt simultation" as in fig. 7? Are they streamflows at the closure section? Here, the name of the figure should be "catchment discharges considering snowmelt", or something like that."

We will change the figure captions to "Stream flow simulations considering different snowmelt models".

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 4063, 2014.