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

Anonymous Referee #1

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Comments on: Modelling of snow processes in catchment hydrology by means of downscaled WRF The presented paper deals with a coupled model approach. Different snow models are driven by a set of parameters delivered by the WRF model. The investigation area is located in the Hartz Mountains (Germany). The general aim of the paper, to introduce a model chain which is independent of surface measurements, is an important research topic. The authors clearly describe the deficits of sometimes sparsely distributed point measurements with respect to spatially distributed models. Hence, they are using WRF fields for driving land surface models. This would be a favorable approach but because of deficits within the precipitation fields they are substi-

tuted by measurements in the course of the paper. This definitely limits the significance of the whole paper and stands in contradiction to title, abstract and introduction. Beside of this the calibration strategy remains unclear and is sometimes inexplicable. It is said that the snow models are calibrated by using WRF fields instead of measurements but precipitation measurements are used for driving the model afterwards. This is hard to understand. Finally, the availability of validation data is extremely limited and the data seems to be inappropriate for checking the quality of different key results of the snow models. If these deficits can be eliminated the paper could be worth for publication.

Line 9: Can you proof this assumption by data or by a citation. Does it matter at all?

P4068 Line 1 pp.: What is the database for the mentioned values? How many stations were available? Where are these stations located? What is the configuration of the stations?

P4068 Line10 pp.: What are the reasons for choosing these two winter seasons? Why have you chosen seasons which are significantly different from the average? Would it make sense to analyze a season which is close to the average as a benchmark?

P4074 Line 3: On the basis of which criteria?

P4074 Line 4: What information have you used for doing this?

P 4074 Line 6pp: Which parameters where calibrated? Which calibration scheme was used, what are the quality criteria's? What was the quality of the model after calibration? Why have you calibrated the model by using met-stations instead of using WRF fields, which are used for the successive model runs? What was calibrated in which snow model? Why have you used WRF fields here and no meteorological stations? Please show why a calibration scheme which id based on two input data sets (meteorological stations for the hydrological part/ WRF for the snow model part) is consistent and applicable.

P4074 Line 21: Please show the network.

P4074 Line 24: Why have you chosen such a generalized illustration of precipitation? If 19 stations area available a spatially distributed illustration would be possible by e.g. showing the Nash Sutcliff coefficient for any station. This would also allow a more precise estimation of the quality of precipitation within the study area. Moreover, it would be also necessary to see if WRF is able to calculate the correct phase of precipitation.

P4075 Line 12: The usage of Nash Sutcliff would be again more meaningful than r^2.

P4075 Line 16: More precise information about the accuracy of the other parameters is needed.

P4075 Line 16pp: This is critical and leads to a negative evaluation of the whole paper. As you said precipitation is a key parameter with respect to snow cover modelling. You mention the problem of the areal representativeness of point measurements in the introduction and mentioning that this is the reason for using models like WRF. But right now you are going in the opposite direction because you are argue that point scale precipitation is better than WRF.

P4076 Line 1pp: The results are hard to interpret. First of all you are discussing the results for the calibration period by using no quantitative measures. Moreover, it is not clear if you have used WRF for the calibration of the snow models (as it was mentioned before) or WRF and measured precipitation which would be more meaningful as you are driving the models in this configuration.

P4077 Line11pp: You are validating your models mainly on the basis of snow melt. It would be good to know which kind of Lysimeter you have used. Moreover, it is well known that the melt rates measured by Lysimeters can be significantly biased. Hence, additional parameters would be needed for estimating the quality of the models (e.g. snow depth, SWE)

P4078 Line 3: How? Which parameters of which models? What are the input parame-

ters?

P4078 Line 7pp: I don't see your calibration strategy. How have you altered the parameters? How have you defined parameter ranges?

P4078 Line 29pp: This is problematic because in the abstract you are talking about an approach able to simulate snow pack and snow melt processes on the catchment scale by using WRF fields. First of all you have excluded the precipitation field and now you are not able to give a measure for the accuracy of the snow pack evolution.

P4079 Line 13pp: I don't think that this kind of evaluation is adequate. The model should be quantitatively validated by available and meaningful parameters. At least everything is guessing in here. It is probably that the inclusion of the canopy stands in ESCIMO improves the whole model as it was shown by Warscher (2013) but they have used a consistent model package and consistent validation data for showing the effect of different model components on the quality of the results. Here we have a highly calibrated model and an improvement can be due to the inclusion of the named effects or we have a typical case for being right for the wrong reasons.

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