General comments

The manuscript presents a method of estimating spatially variable degree day factors (DDFs) based on snow-covered area given by MODIS, ground based measured and interpolated snow depth, precipitation and air temperature data. Although the method is inevitably connected with uncertainties, the idea is worth to be published. The approach is described clearly enough to be used by other scientists. DDFs estimated by the method are used in a hydrological model. Detailed description and discussion of the results obtained by modeling based on two different ways of DDFs estimation is presented. The discussion is sometimes too detailed to my taste. However, some readers may find it useful, therefore I do not propose any changes regarding this. The results do not prove significant improvement when using the spatially distributed DDFs obtained by the proposed method. Despite that I believe that hydrological modeling at certain scales should be better based on DDFs obtained by the proposed method than only calibrating the DDF as one of model parameters. The reason is that under favorable conditions, the spatially distributed DDFs obtained by the proposed method may be closer to the reality, i.e. to water volumes released from snow during snowmelt. They are physically better justified compared to DDFS obtained just as calibrated model parameters. Under "certain scales" mentioned above I mean catchments that are large enough considering the MODIS resolution and small enough to make the interpolation of other input data reasonable.

Reply: Thank you very much for your careful review and detailed comments. The modeling improvement when using the spatially distributed DDFs obtained by the proposed method should indeed be different for different modeling scales. The modeling scale, i.e. size of fundamental computational unit (sub-catchment in this study), can have a significant influence on the simulation, considering the spatial resolution of MODIS data and the spatial density of gauge stations for precipitation and temperature. Adopting different sub-catchment sizes in the model could be a potential way to analyze the scale effect on the simulation, which can be an issue for further study. We have added this discussion in the revised manuscript.

We have taken your following comments into account, and revised the manuscript accordingly. Detailed replies to your comments are as follows.

Specific comments: I have the following comments which address rather modeling and other issues than the method of distributed DDFs estimation itself:

1. Section 2.3. and elsewhere – I propose to avoid using the term "validation of

estimated DDFs". The word "validation" is confusing. Because the true DDFs values are not known, they can not be validated. Comparison of runoff and snow pattern simulations with DDFs obtained by two different ways is not validation of the DDFs. In other words, similar values of simulated runoff and snow patterns do not guarantee that DDFs, i.e. volumes of water released per degree-day are the same as the ones observed in the nature. Fig. 9 presents a nice example that runoff simulation may be acceptable even if the snow-covered area during the snowmelt (which depends also on spatial differences in melting, i.e. the DDFs) is different from the reality.

Reply: The concept of "validation of estimated DDFs" has been removed and replaced with the concept of "evaluation of estimated DDFs".

2. Use of precipitation and air temperature data from the whole Austria to interpolate values for a relatively small basin in its southern/south-western part is in my opinion not needed. Data from smaller territory around the studied catchment would presumably provide better description of local climatic conditions in further studies.

Reply: Yes, the precipitation and air temperature data were interpolated by the external drift kriging method, which takes into account the local relationship between variables and altitude. The local radius was set to the distance found in geostatistical analysis and it is typically between 50 and 80 kilometers. We thus believe that such an approach can represent the local basin characteristics and allows estimating model inputs for each of 95 sub-catchments in an objective way.

3. I recommend using "baseflow" instead of "groundwater baseflow". Although no unique definition of baseflow is accepted in hydrology (many different definitions exist), baseflow generally characterizes sustained streamflow during dry periods. Expression "groundwater baseflow" is confusing, because it might imply that groundwater flow is known (which is rarely the case) and that only part of that groundwater flow is defined as groundwater baseflow.

Reply: Revised according to the suggestion.

4. Stepwise calibration might be an alternative calibration approach that some readers may find interesting. However, a more detailed inspection of Figs. 5 and 6 shows that the hydrological model quite often does not simulate the streamflow at the beginning of the snowmelt season very well (2001, 2004, 2006, 2008, 2009, 2010). The model needs some time to simulate increased streamflow or an event. It is not an uncommon behavior, but further development of the model may consider this issue.

Reply: We acknowledge that the simulation of the streamflow events at the beginning of the *spring season is not always very good. Reasons for this behavior include underestimated soil storage and underestimated snowmelt water at the beginning of the year. In further studies, the exact reason for the low performance of these early events should be diagnosed, and the model should be improved.*