We thank Salvatore Manfreda for acknowledging the importance of the topic addressed in our paper. He raised several supportive and helpful comments on our paper. Below we address each comment and explain how we revised the manuscript in response:

1. The paper is too long and needs to be shortened. There are several repetitions that must be removed. I suggest to remove the PC analysis and to condensate the results and the discussion sections in only one section.

   We agree with the reviewer that the paper can be shortened. The overall aim is to identify soil moisture pattern types and their relationship to observed flood events. The methodology is based on a principal component analysis (PCA) to reduce the data dimensionality and a subsequent cluster analysis on the leading principal components (PCs) to identify days of similar soil moisture patterns. The result of the cluster analysis depends (1) on the outline of the PCA and (2) on the number of PCs in the cluster analysis. For this reason, we prefer not to remove the PCA analysis completely. Instead, we propose to remove the sections concerning the PC interpretation (page 10064 line 3-15, page 10069 line 13-22, page 10071 line 9-19, figure 5, figure 8). This will reduce the length of the paper considerably and put a stronger focus on the soil moisture pattern classification. Additionally, we limited Figure 4 to 4 PCs and integrated the information provided by Figure 3 for these PCs in the text.
   We prefer to separate strictly between results and discussion and feel that short repetitions of the main results might facilitate the comprehensibility of the discussion. Nevertheless, we have carefully rechecked the manuscript for repetitions.

2. Analyses presented here are based on modelled data and it is absolutely necessary to verify the reliability of the results. In particular, the hydrological model adopted has a significant number of parameters that may be easily tuned in order to obtained an acceptable response. Now, the problem in these cases is very well addressed in the paper of Kirchner (2006) that says “advancing the science of hydrology will require not only developing theories that get the right answers but also testing whether they get the right answers for the right reasons”. So my question is the following: Are you sure that the model perform well for the right reason? A correct interpretation of the stream flow does not necessarily implies a good interpretation of the soil moisture patterns.

   Here, the reviewer raises an important point. Simulated soil moisture depends on (i) climate and soil data as well as (ii) model parameterization and structure. Applying a Monte Carlo uncertainty analysis, we accounted for the uncertainty related to model parameterization. Nevertheless, the influence of climate and soil data as well as the representation of processes was so far not addressed. A verification of the simulated soil moisture with soil moisture point measurements (e.g. gravimetric, TDR) is not feasible as these are highly variable over short distances. On the other hand, satellite based and hydrological simulated soil moisture estimates are spatially integrated values. For this reason, we validated the temporal progression of simulated soil moisture against the remotely sensed soil water index (SWI) (Wagner et al., 1999, http://www.ipf.tuwien.ac.at/radar/ers-scat/home.htm) by calculating the Pearson correlation coefficient between the standardized simulated soil moisture (SMI) and the SWI. Figure A shows the Pearson correlation coefficient (significant at 5 % level) between the median simulated SMI and the remotely sensed SWI. Except for a few local spots, basin wide high correlations are observed. The median correlation is 0.57, the difference between the 25th and 75th quantile is 0.2. The individual examination of the Monte Carlo parameterizations leads to the same findings. Hence, *we demonstrated that the simulated temporal progression of the SMI is well represented in the hydrological model.*

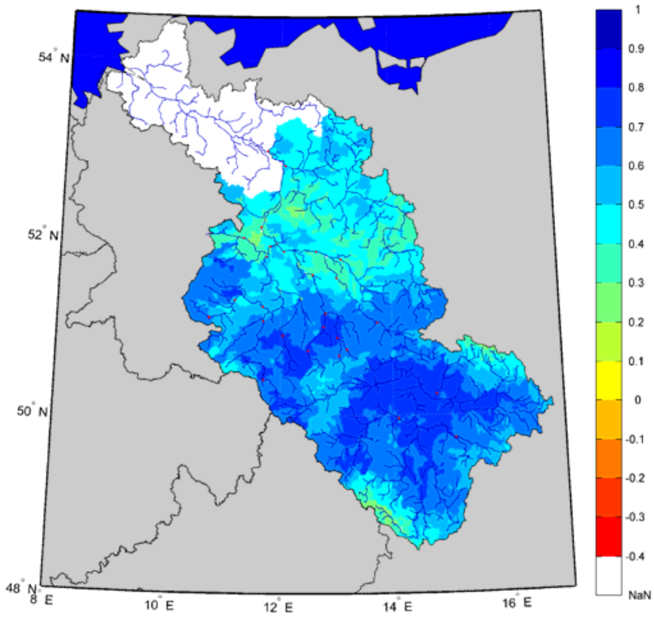

Figure A. Pearson correlation coefficient (significant at 5 % level) between the median simulated SMI and the remotely sensed SWI.

We included this validation in the revised manuscript. In the 'Data' section, we added a brief description of the retrieval of the soil water index and data availability. The Pearson correlation coefficient as well as the assignment of the SWI to the subbasins is explained in section 'Methodology: Model calibration and validation'. The results including Fig. A are presented in 'Results: Hydrological modeling' and discussed in the subsequent section.

This validation approach doesn't yet address the possible existence of model structural errors. However, Huang et al. (2010) compared the spatial patterns of the annual average actual evapotranspiration, total runoff and groundwater recharge between SWIM and the Hydrological Atlas of Germany (HAD) for the period 1961-1990. The comparison of the two different data sources revealed a good representation of the spatial patterns of the water flow components in the SWIM model.

3. One problem that I think may affect soil moisture description at the local scale is the use of the CN method that is a method developed at the basin scale but not reliable at the local scale.

    In the hydrological model, surface runoff is estimated by applying the CN-method on the hydrotope scale to account for the infiltration characteristics of different soil and land use types. In accordance with the area share of each hydrotope, subbasin average surface runoff is calculated. This approach doesn't account for local effects as e.g. re-infiltration due to a change in land use characteristics. However, we are interested in the soil moisture at the subbasin scale (2 km² - 1034 km²). Therefore, *local processes can be neglected and the CN-method is a suitable approach for our work.*

4. Page 10058: Modelling assumptions may impact significantly on the obtained simulated patterns. The use of two markedly differ soil maps have an impact that one may recognize in the observed patterns of the principal components (see PC2). This is one question that should be addressed in the paper and that may produce misleading results.

   The major soil type classifications are similar in the German soil map 'BUEK 1000' and the FAO-UNESCO soil map. However, there are more detailed parameters available for the German part which account for the land use types from CORINE. Since CORINE is available at the European scale, we used a similar parameterization for the Czech part.
   Looking at the spatial distribution of the Person correlation coefficient between simulated and remotely sensed soil moisture (Fig. A), there is no obvious difference in the correlations between the Czech and the German part of the basin. Therefore, we exclude an impact of the different soil maps and climate station densities on the temporal progression of simulated soil moisture. Having a closer look at the loadings corresponding to PC2 (Fig. 4), even though there is a clear North-South partition, this boundary is not identical to the political boundary between Germany and the Czech Republic.
   We included a brief comment stating that no effect of soil data and climate station density is visible in the Pearson correlation coefficient (Fig. A) in the section 'Results: Hydrological modeling'.

5. The authors state that the model provides a significant overestimation of the runoff (28%-40%) (see page 10068 lines 7-9). This result is due to the fact that the model was optimized in order to provide a correct description of flood events obtaining as a consequence significant errors in the water balance. This may affect the estimation of evapotranspiration and also the resulting soil moisture.

   Based on the model validation (see comment 2)), the simulated soil moisture is reasonable for our purpose.

6. page 10060 line 22-24: The term soil transpiration is inappropriate. Use the term soil evaporation.

   We agree and use the term soil evaporation in the revised version.

7. page 10067 line 1-3: Please provide a description of how the start dates of flood event were identified.

   We identified food events using an approach proposed by Uhlemann et al. (2010). The flood start date is defined as the date where the first gauge in the catchment shows a significant peak. This flood start date is up to three days in advance of an observed 10-yr flood in the catchment. We provide the definition of flood start dates in section 'Methodology: Flood event identification'.

8. page 10068: It would be extremely useful to provide a list of the calibrated parameters.

   We have added a list of the calibrated parameters in the revised manuscript. "Nine sensitive parameters controlling snow accumulation and melt, potential evapotranspiration, saturated hydraulic conductivity, recharge as well as discharge routing are calibrated over the period 1981-1989" (page 10061 line 16-18).

   Table A: Model parameters and their calibration range.

Model parameter	*Description*	*Calibration range*
tsnowfall	*Snowfall temperature [°C]*	*-1.0 - 2.5*
tmelt	*Snowmelt temperature [°C]*	*0.0 - 3.0*
snowmeltrate	*Melting rate [mm d^{-1}]*	*1.0 - 4.0*
thc	*Correction factor for potential evapotranspiration on sky emissivity [-]*	*0.1 - 1.2*
sccor	*Correction factor for saturated conductivity [-]*	*0.1 - 20.0*
abf0	*α-factor for groundwater [-]*	*0.0 - 1.0*
bff	*Baseflow factor [-]*	*0.0 - 2.0*
roc2/roc4	*Routing coefficients surface runoff [-]*	*0.1 - 15.0*

9. Page 10068 The authors use the term soil moisture profile, but you probably they refer to soil moisture map.

   Yes, we refer to the soil moisture map and have corrected the manuscript.

10. page 10068 line 23-26: The authors highlighted the differences observed in the PC2 with respect to the German and Czech part of the basin, but neglect to say that this may be due to the parameters set used for the soil map (see may previous comment).

    Please refer to the reply on comment 4).

11. Figure caption are not self containing. It is very hard to understand the contents of the graphs from captions. Please try to provide more detailed descriptions.

    In the revised version, we changed the captions the following:

    Figure 1: *Topographic map of the Elbe river basin. Yellow dots: Discharge gauges applied in flood identification. Red dots: Discharge gauges applied in flood identification as well as model calibration and validation. Crosses: Location of pixel centroids of the scatterometer data.*

    Figure 4: *Loadings (left) and their corresponding PCs (right) of the leading four PCs. PCs are displayed for the sub-period 1982-1991. Minimum and maximum values correspond to the parameter uncertainty introduced by the rainfall-runoff model.*

    Figure 6: *Median probability of cluster membership p_t of different PC-cluster combinations (left). PC-cluster combinations with a small median p_t are strongly influenced by model parameterization.*
    Distribution of p_t for different numbers of clusters when clustering the leading four PCs (right).

    Figure 7: *Soil moisture index (SMI) patterns of cluster centroids.*

References:

Huang, S., Krysanova, V., Österle, H., and Hattermann, F. F.: Simulation of spatiotemporal dynamics of water fluxes in Germany under climate change, Hydrol. Process., 24, 3289-3306, 2010.

Uhlemann, S., Thieken, A. H., and Merz, B.: A consistent set of trans-basin floods in Germany between 1952 - 2002, Hydrol. Earth Syst. Sci., 14, 1277-1295, 2010.

Wagner, W., Lemoine, G., and Rott, H.: A method for estimating soil moisture from ERS scatterometer and soil data, Remote Sens. Environ., 70, 191-207, 1999.