Response Letter

The value of satellite soil moisture and snow cover data for the transfer of hydrological model parameters to ungauged sites

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In the following document, we reproduce all the comments of the Referees in italic characters followed by our responses in blue.

Response to referee #1

The study of Tong et al. focuses on testing the possibility of improving the hydrograph prediction in ungauged basins, by adding ASCAT soil moisture and MODIS snow cover data to runoff. For this aim, the study applies multi-objective calibration with changing weights between soil moisture, snow cover and runoff. Coupling the TUW model with eight typical regionalization methods, this study compares the differences and impacts of adding soil moisture and snow cover data from three aspects in 213 assumed ungauged Austria basins. The authors conclude that the calibration variant has a larger impact on runoff prediction accuracy than the selection of regionalization methods in ungauged catchments. Overall, the authors present a thorough analysis, the results seem convincing and the study is valuable for related research.

We want to thank the reviewer for her/his very positive assessment of the manuscript.

However, there are several issues that still exist and need to be clarified further as indicated in the following.

First, the manuscript needs further editorial work to improve the paragraph structure and some vague expressions. The results section, figures 2-4 and tables 2-4 evaluate the prediction from two different aspects (median value and the 50% confidence interval, respectively), the text is thus suggested to set in two separate paragraphs. In addition, please pay attention to vague expressions in this manuscript, such as line 394 "This study suggests that the future evaluation of the transfer of model parameters to ungauged sites will benefit from examining what type of information will improve the calibration and transfer of model parameters related to the runoff generation and routing", which is really confusing. There are other similar sentences, so I hope the authors make a thorough change to improve the clarity of the manuscript.

Another major issue in this manuscript is that the Results section can be made more concise and to-the-point. Information presented in Figures 2-4 and table 2-4 includes both calibration and *validation results, which are mostly similar, and limited text for validation result presented in current version. Thus, I would suggest that the authors focus more on one of the cases and improves the presentation of figures and tables to make sure the key messages stand out. Moving the validation information to the Supplement may be an option.*

Thanks for the suggestions. In response to this comment, we will improve the paragraph structure and revise the vague statements as suggested by the reviewer. We agree that the results for calibration and validation are similar. The reason for presenting both is to examine the split-sample performance as suggested by Klemes (1985, https://doi.org/10.1080/02626668609491024) and we will state this more explicitly in the manuscript. We will also improve the presentation of figures and tables to make sure the key messages stand out.

Furthermore, the conclusions are mixed with discussion in current version, which is not easy for the readers to get the key messages from the study. I would suggest that the authors conclude the findings in a separate section, and make more concise and clearer conclusions.

We will separate the conclusions from the discussion as suggested by the reviewer.

To conclude, I generally like the approach and methodology, but some moderate improvements are needed. I hope the authors find my comments useful and I am looking forward to an improved version of the manuscript.

We consider the comments indeed very useful. Thank you.

Technically I have a couple of comments for current version:

(1) L138: "...with cloud cover less than a threshold 50%." Is 50% a subjective value? If so, please clarify the reason, otherwise, add the reference.

This threshold was chosen on the basis of the sensitivity analysis performed by Parajka and Blöschl (2008). In response we will add “The thresholds of ξ_{SWE}, ξ_C, and ξ_{SCA} were determined by the sensitivity analysis of Parajka and Blöschl (2008).” at the end of this paragraph.

(2) L140: "... over a threshold of 25% in the zone." The same comment as above.

Please see the response above.

(3) L204-206: "...between climatic zones...the catchments were split into two groups...elevation below 900 m a.s.l. ... elevation above 900 m a.s.l.". The reference is the climatic regions, but the classification in this study is only based on elevation. Please make a clarification here, for instance, adding a table presenting the climatic statistics between two groups.

Thanks for the suggestion, in response to this comment we have prepared a table showing the climatic statistics for the two groups which will be added to the appendix of the paper and referred to in the main text.

Table S.1. Statistics of the climatic attributes of the 94 lowland catchments and 119 alpine catchments. With abbreviation, unit, minimum, maximum, and median. The standard deviations refer to spatial variability within each catchment.

Attribute	Abbrev.	Unit	Lowland (mean elevation under 900 m a.s.l.)			Alpine (mean elevation over 900 m a.s.l.)		
			Min.	Max.	Median	Min.	Max.	Median
Mean annual precipitation	MAP	mm	728.13	1828.40	999.46	913.66	2301.84	1476.64
Standard deviation of annual MAP	SDAP	mm	10.79	367.57	71.49	30.13	289.87	152.90
Mean air temperature	MAT	°C	7.26	10.30	8.98	-2.83	8.07	5.76
Standard deviation of MAT	SDAT	°C	0.06	1.71	0.57	0.40	3.55	1.64
Mean annual potential evaporation	MEPI	mm	618.36	740.45	690.08	233.49	657.01	563.00
Standard deviation of MEPI	SDEPI	mm	4.33	77.41	25.25	21.70	162.07	83.33
Catchment aridity index (MEPI/MAP)	CAI	-	0.36	0.98	0.66	0.18	0.69	0.37
Standard deviation of aridity index	SDAI	-	0.01	0.31	0.06	0.02	0.18	0.09

(4) L249: "Besides, to exclude invalid ASCAT measurements ... or snow cover exceeds 30 % of the pixel." Vague expression, please modify.

We modified the sentence as "Besides, to exclude invalid ASCAT measurements affected by snow and frozen ground, soil moisture is masked as no data when soil temperatures at a soil depth of 0-7 cm are below 1°C or snow cover exceeds 30 % of the pixel with the information from the ECMWF Copernicus Climate Service (C3S) ERA5-Land."

(5) Results: The model performance is missing. Please add a figure or table showing the assessment of model simulation accuracy in calibration and validation period. At lease, show some general information in text.

In response to this comment, we will indicate the at site calibration performance in the figures and text, as suggested by the reviewer. We will also add an evaluation of the loss of performance of the different regionalization methods.

(6) L256: "The results for the runoff weight =1.0 represent ... without using observed runoff." This information is repeated, may delete it here.

Thank you, we will remove the repetition.

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(7) L259-261: "..., for weights below 0.4. ... larger than 0.4." It is not easy to see the difference before and after 0.4 from Figure 2, please modify the text or figure.

In response to this comment, we will modify Figure 2 and add X axis grid lines. We believe this will indicate the position of the performance for the selected weights more clearly.

(8) L259-261: "... In this case, ...". Here "this" is confusing, please modify the expression.

We will modify the sentence as follows: "For wQ larger than 0.4 the differences between the transfer methods are larger, …"

(9) L275: "The largest difference occurs ...". Please clarify "difference" here.

We will add “between the local and global methods” after “difference”.

(10) L276: "An exception is ...". Please add reasons after this sentence.

In response to this comment, we will add the following explanation: “An exception is the regression of model parameters, which has a larger runoff efficiency for the global than the local approach. The reason is a larger correlation between model parameters and catchment attributes estimated from all catchments. For example, for wQ=0.4, the median of the correlation between model parameters and catchment attributes for the local regression varies between 0.22 and 0.65. For the global regression approach, the median is larger and varies between 0.70 and 0.88.”

(11) L305: "... Also here, ...". Vague expression, please modify.

We will modify the sentence as follows: “The best transfer methods in alpine catchments are local and global similarity and kriging. The median correlation between modelled and satellite soil moisture is however small and varies between 0.14 and 0.22.”

(12) L320: "The results indicate the smallest difference in snow efficiency between the transferred methods". What or who is " the smallest"? please clarify and modify the expression here.

We will modify the sentence as follows: “The results indicate that the variability and differences between the regionalization approaches are the smallest for snow efficiency.”

(13) L321: "A much larger difference...". Please add information about the comparison components.

We will modify the sentence as follows “A much larger difference and impact on snow efficiency has the runoff weight used in model calibration.”

(14) L323: "... between 1 and 3% ... between 8 and 17%." How did you derive this conclusion? Fig 4 shows the transfer methods individually, that readers cannot obtain this information. Please add more text information or modify the figure.

We compared how the snow efficiency varies between regionalization methods and/or between the different runoff weights used in model calibration. The difference/variability in the median snow cover efficiency obtained by different regionalization methods, but for the same runoff weight (used in model calibration) is smaller than the difference (variability) in median snow model efficiency obtained by individual regionalization method across different runoff weights. For example the median of snow efficiency for the runoff weight 0.4 varies between eight groups of regionalization methods between 0.72 (local regression) and 0.74 (global mean) in the alpine and between 0.88 (local regression) and 0.91 (global regression) in the lowland catchments. This variability in medians is about 3%. The variability in medians for one regionalization approach (e.g. kriging) is between 0.87 (runoff weight 1.0) and 0.96 (runoff weight 0.0), which is in relative terms approximately 9% variability. In order to allow a more direct comparison of the efficiency values for the same runoff weight or differences between runoff weights we will modify the Figure 4 by adding gridlines, showing more precisely the efficiency values for different runoff weights.

(15) L326: "... regional variability...". Please clarify its definition.

Regional variability refers to differences in model efficiency between the catchments. We will introduce this term in section 4.

(16) L342: "... Positive efficieny values...". Please add efficiency information before this sentence, in order to connect the figure and text information.

We will rephrase the paragraph as follows: "We compared the efficiencies of the predictions obtained by transferring model parameters from multiple-objective calibration (i.e. wQ<1) with those obtained by parameters calibrated to runoff only (wQ=1). The results are shown in Fig. 5 and Fig. 6 for the calibration and validation periods, respectively, in terms of the relative improvement in snow cover (x-axis), soil moisture (y-axis) and runoff (colour of symbol) efficiencies for eight transfer methods (panels) and ten calibration weights (symbol size) in the lowland (left) and alpine (right) catchments. Positive efficiencies indicate an improvement when using a multiple-objective calibration compared to a runoff-only calibration. The figures suggest that the runoff predictions are very similar (i.e. within a 1% range) if the runoff weight is larger than 0.5 …"

(17) L347: "... very similar (i.e. within 1% range)...". How can the readers derive this conclusion? The legend unit in the figure is 5%.

In response to this comment, we will modify the figures to show the interval between 0-1%.

(18) L355: "... and the improvement is larger in the alpine than the lowland catchments". In my opinion, this is an important and the most obvious finding in Figure 5 and 6, I would suggest to modify the text with an emphasis on this conclusion.

We agree about the importance of these findings. In order to highlight them even more we will add the following sentence: "The patterns of improvement are very consistent with those obtained for the transfer of model parameters in the validation period. The most noticeable finding is that the improvement in soil moisture and snow cover increases with decreasing runoff weight and the improvement is larger in the alpine than in the lowland catchments."

(19) L369: "... the improvement is largest..." → "... the improvement is large..."

Thanks, we will modify the phrase as suggested.

(20) L370: "... of the efficiencies of the different..." → "... of the efficiencies between different..."

Thanks, we will modify the phrase as suggested.

(21) L375: "... we examined all 30 transfer approaches … fewer than tested in Parajka et al. (2005)." This information is not really relevant in discussion, delete maybe.

Thanks, we will delete this part as suggested.

(22) L394: This paragraph is supposed to conclusion section, the expression is not precise and clear enough for readers in current version. Please pay more attention in the logical expression and modify the conclusions more precise and clearer.

In response to this comment we will revise the paragraph as follows:

This study shows that the recent advances in remote sensing of water balance components contribute to improving the hydrological predictions in ungauged catchments. The main improvements are in estimating soil moisture and snow cover dynamics, mostly in alpine catchments. Future analyses may focus on assessing the value of satellite data for other types of regionalization approaches, such as regional calibration (Parajka et al., 2007) or multi-scale parameter regionalization methods (Samaniego et al. 2010, Kumar et al. 2013). It will also be interesting to evaluate how much runoff information is needed in addition to existing satellite products to improve and constrain the model predictions in ungauged basins. Such investigation can also include an analysis of the role of nested catchments in parameter transfer and the impact of stream gauge density on the regionalization model performance."

Updated Figures:

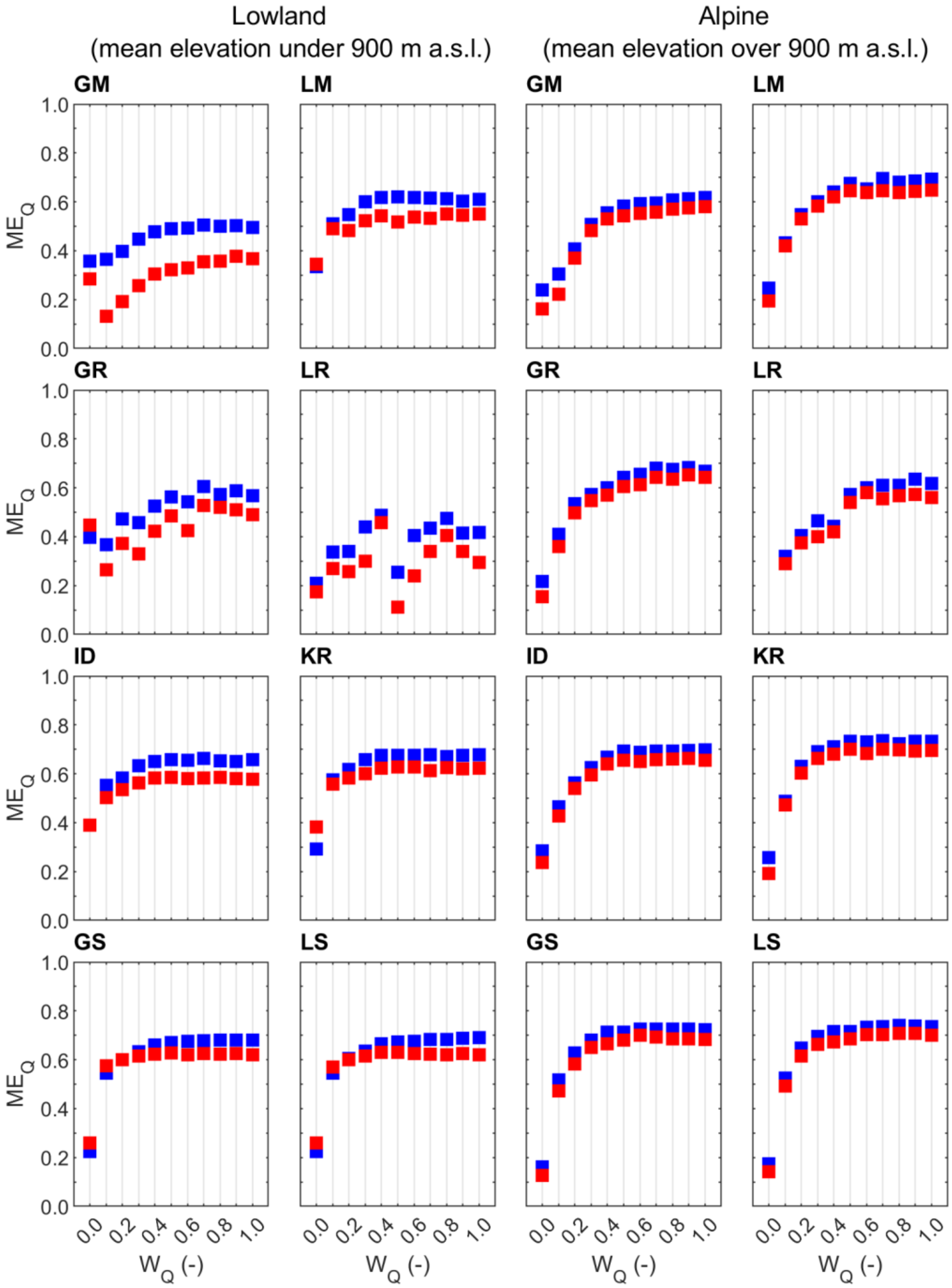

Figure 2: Median of leave-one-out runoff model efficiency (Eq. 5) obtained by eight groups of parameter transfer methods and eleven calibration weights for lowland (94) and alpine (119) catchments in the calibration (2000-2010, blue symbols) and validation (2010-2014, red symbols) periods.

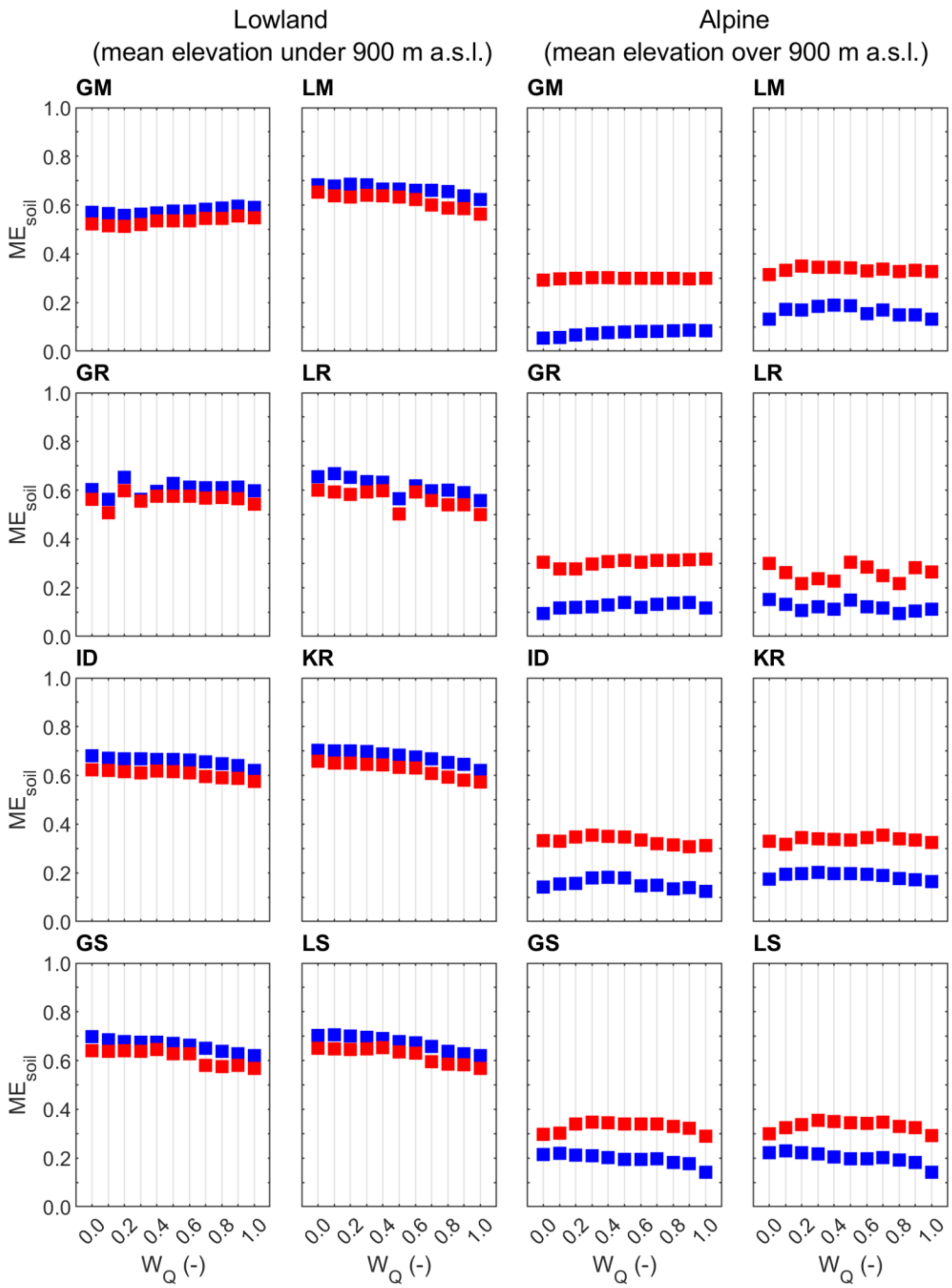

Figure 3: Median of leave-one-out soil moisture correlation (Eq. 8) obtained by eight groups of parameter transfer methods and eleven calibration weights for lowland (94) and alpine (119) catchments in the calibration (2007-2010, blue symbols) and validation (2010-2014, red symbols) periods.

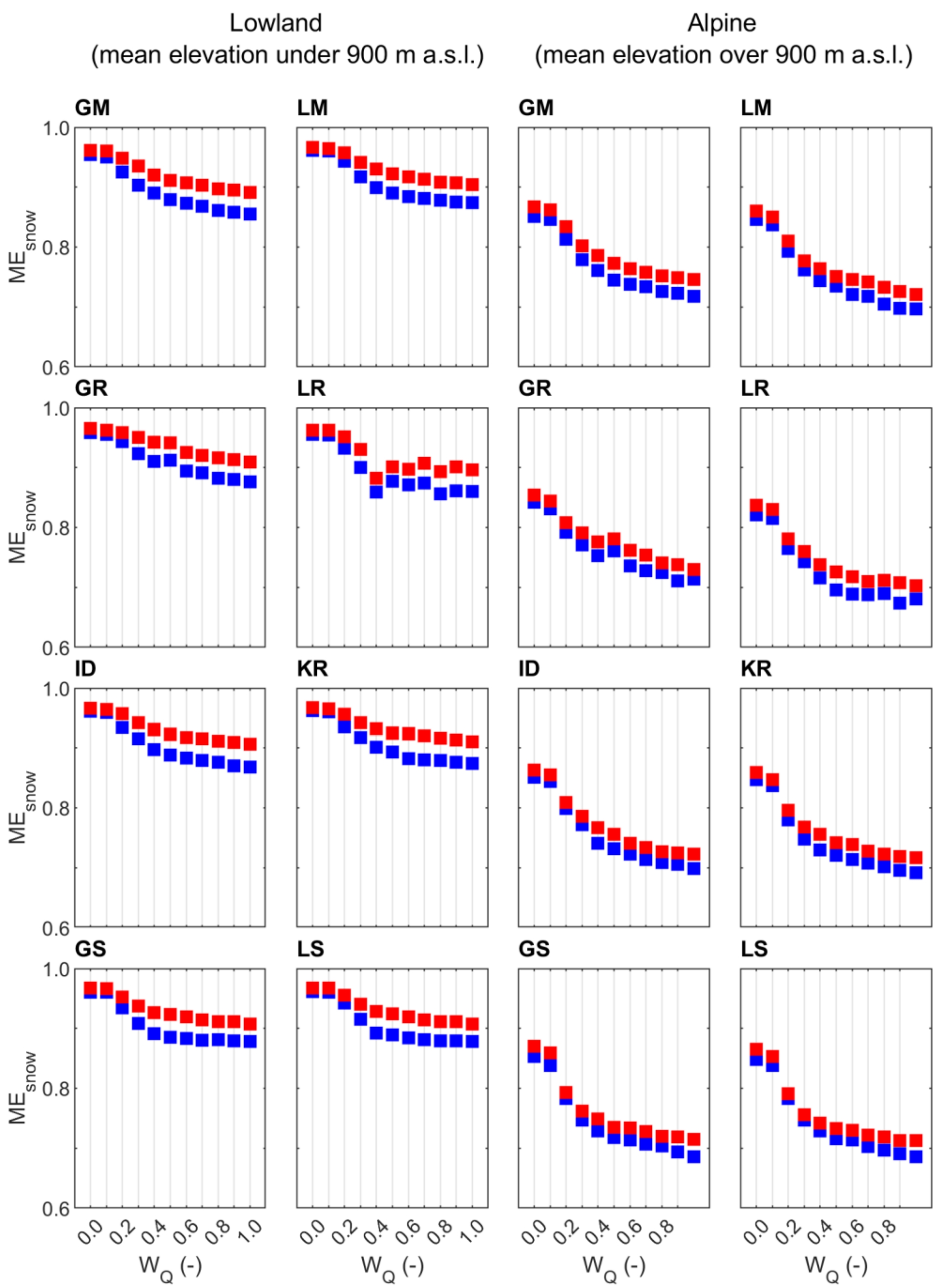

Figure 4: Median of leave-one-out snow model efficiency (Eq. 9) obtained by eight groups of parameter transfer methods and eleven calibration weights for lowland (94) and alpine (119) catchments in the calibration (2000-2010, blue symbols) and validation (2010-2014, red symbols) periods.

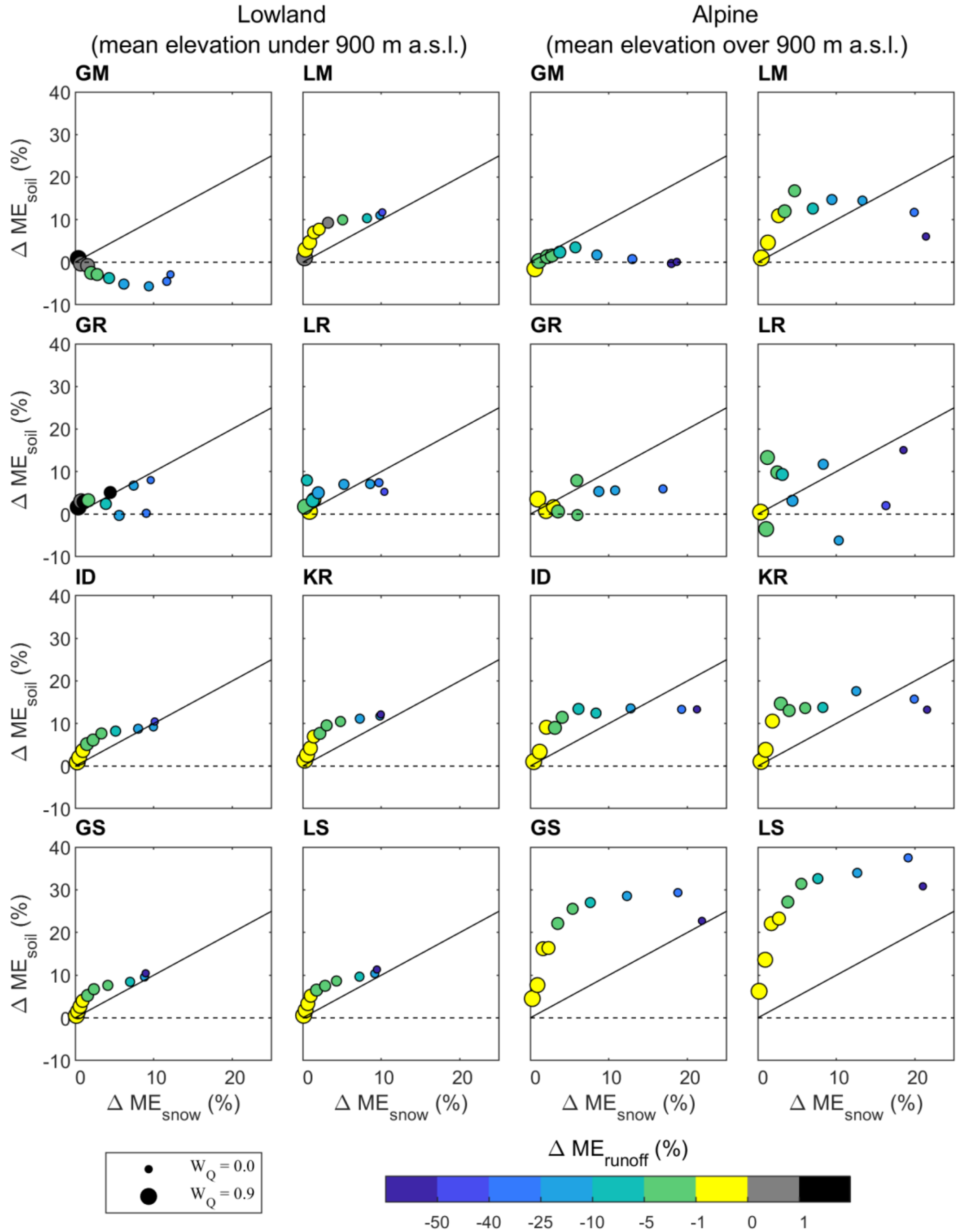

Figure 5: Relative difference (%) in the median of snow cover (horizontal axis), soil moisture (vertical axis) and runoff (colour of symbols) efficiency between model simulations obtained by transferring model parameters calibrated by multiple-objective calibration and calibration to runoff only. The relative difference is estimated for eight model transfer methods (panels) applied in the lowland (left panels) and alpine (right panels) catchments in the calibration period 2000-2010 (soil moisture: 2007-2010).

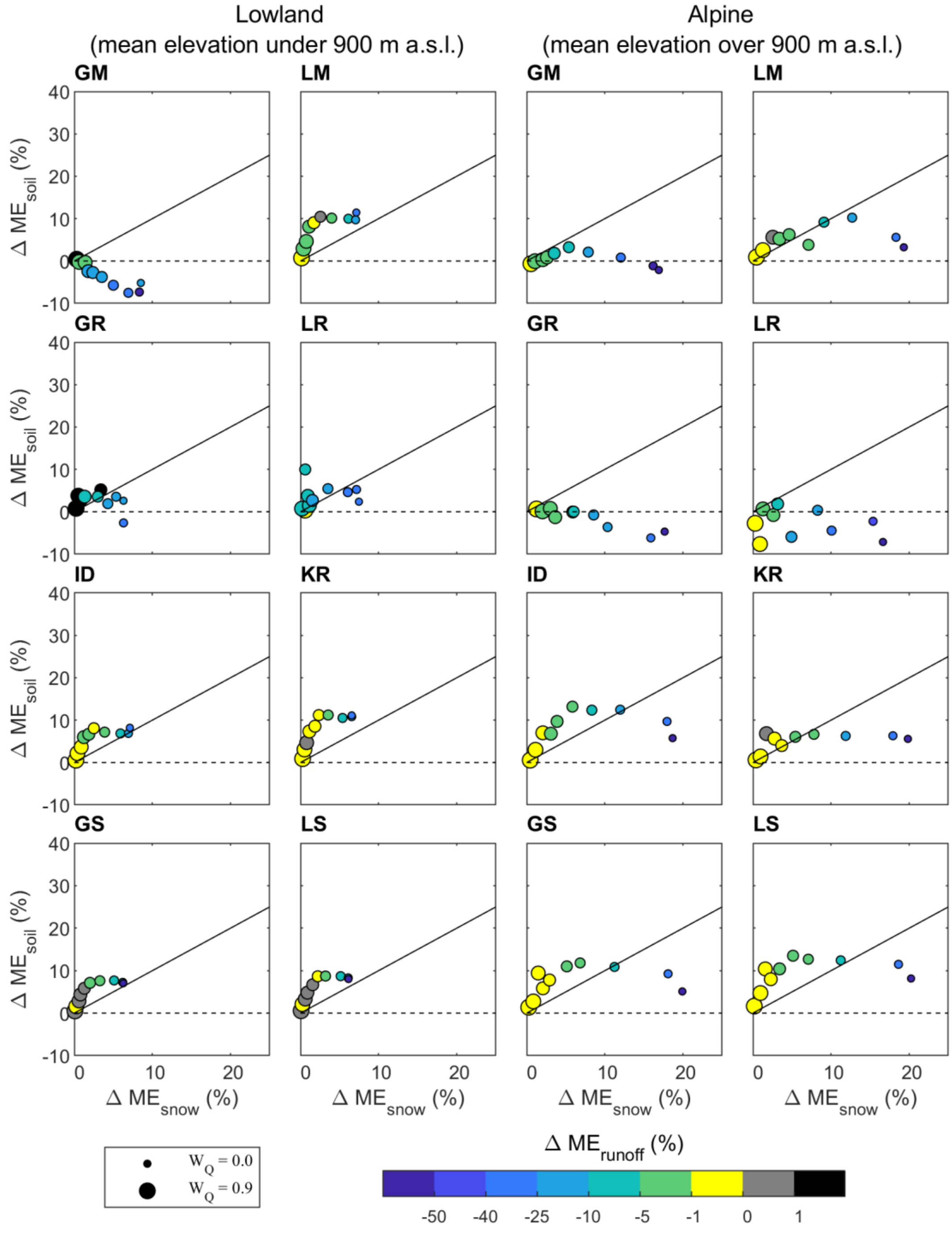

Figure 6: Same as Fig. 5, but for the validation period (2010-2014)

Response to referee #2

General Comments

This manuscript aims to evaluate the efficiency of parameter several regionalization techniques applied to a hydrological model tested in several sites in Austria. The calibration procedure uses multi-objective functions. The authors expect to improve predictions of streamflow, soil moisture and snow cover in ungauged locations.

The primary motivation of the paper is not only to evaluate the efficiency of selected regionalization methods, but (i) to test the benefits of using regionalization of model parameters obtained by multiple-objective calibration and (ii) to validate the regionalization performance by using additional hydrological characteristics to streamflow, as stated in the manuscript (page 3, line 69). There are hundreds of studies evaluating different regionalization methods, but the assessment of using multiple objectives in a regionalization framework is still rare.

This topic is an active area of research in hydrological and land surface modeling. This manuscript in its present status, however, is not suitable for publication in HESS. The major issues I have with this study is its lack of innovation compared with many past studies on this important subject.

While we agree that this is an active area of research, we strongly disagree regarding the lack of innovation. Specifically, the present work goes beyond existing studies (including those cited by the reviewer) in:

- Evaluating regionalization efficiency of model parameters calibrated against runoff and remotely-sensed derived soil moisture and snow cover, including the impact of weighting of different objectives.
- Examining the value of the recently developed soil moisture S1ASCAT dataset (ASCAT downscaled with Sentinel 1 data) and the new version of the MODIS snow cover dataset for constraining and validating hydrologic model regionalization.

We believe that the regionalization of model parameters constrained by satellite data of soil moisture and snow cover combined with multiple-objective validation of regionalization approaches is still part of an unsolved hydrological question and this paper makes a novel contribution to it.

In addition to that, authors pay no attention to a sound analysis of uncertainty that is needed to be able to have conclusive evidence.

We again disagree here and believe that we do conduct a sound uncertainty analysis. There are different sources and aspects of uncertainty which can be evaluated. We evaluate the regionalization efficiency by using the split sample uncertainty assessment proposed by Klemes (1985, https://doi.org/10.1080/02626668609491024). The study also evaluates the impact and uncertainty related to the magnitude of runoff weight used in the multiple objective calibration. We believe that these are the most important sources of uncertainty in this context and therefore prefer to focus on these.

The Authors have done also a poor job documenting the state of the art in this subject and do not compare their results against existing regionalization methods.

> Prediction in ungauged basins (PUB) is a research field that has produced hundreds of publications in the last decades, which have already been summarized in review publications such as Blöschl et al. (2013). The introduction section thus focuses on the more specific topic of the paper, i.e. the application of a multiple-objective framework in regionalization which involves a much smaller number of previous studies. The reviewer indicates numerous studies that he recommends to be cited. Most of these studies are, however, covered in the already cited review studies, and the majority of them is not directly related to the objectives of the study. For example, Mosley (1981) examined the clustering of selected catchments in New Zealand to identify the similarity in flood regimes and Fernandex et al. (2000) evaluated the regional calibration of a monthly water balance model, which are part of the more general PUB problem, but do not provide information on multiple-objective regionalization which is addressed in the present paper.
>
> In our analysis we compare eight groups of existing regionalization methods, but comparing all methods in the literature would of course be beyond the scope of the paper. We are aware of the MPR method of the reviewer (and other methods not mentioned) and it will be interesting to compare them in future analyses. In the discussion section we will include a comparison of the regionalization performance with the most similar methods from the literature (even if identical methods do not exist) where consistent performance measures are available.

Moreover, this study is not driven by a rigorous hypothesis and hence I cannot see a clear experimental design that would lead to significant conclusions that can be applicable somewhere else.

> The reviewer seems to imply that a meaningful hydrological analysis requires the statement of a hypothesis. A recent debate in Water Resources Research (Blöschl, 2017; doi:10.1002/2017WR020584) suggests that many colleagues do not agree with this sentiment and open science questions (such as those stated in this manuscript) are considered equally valid, because many interesting questions cannot be captured by a binary reject/not reject alternative. We believe that the experimental design is indeed clear, as outlined in the methods section, and the findings are robust and transferable to other regions given that the analysis is based on a large sample of catchments (213 in total) with hydrological conditions ranging from lowlands to alpine.

Specific Comments

This manuscript has the following major technical shortcomings:

- *The work on Regionalization in hydrology and land surface modeling is quite substantial. In the work of Samaniego et al 2010 (WRR), for example, the following works were already mentioned: [Mosley, 1981], [Abdulla and Lettenmaier, 1997], [Seibert, 1999] [Hundecha and Bárdossy, 2004], [Götzinger and Bárdossy, 2007], [Pokhrel et al.,* *2008], [Kim and Kaluarachchi, 2008], [Fernandez et al. 2000], [Troy et al, 2008]. From these old works only [Parajka et al., 2005] is mentioned.*

As mentioned above this paper is not about regionalization and land surface modeling but instead about a much more specific question. Those suggested references that are relevant are already included in the review studies referred to in the manuscript (i.e. He et al., 2011, Blöschl et al., 2013, Hrachowitz et al., 2013, Parajka et al. 2013, Guo et al., 2021), and we find little value in citing studies that only refer to the regionalization topic in general.

- *The method proposed in [Samaniego et al. 2010] was called MPR. It uses regionalization or regularization equations (i.e., pedo-transfer functions derived from soil physics) and scaling operators (averaging rules), also proposed by soil scientists. The concept is clearly explained again in [Samaniego et al HESS 2017]. Now even exist a stand alone method to perform these tasks in any land surface model.*
  See Schweppe et al 2021 GMD https://doi.org/10.5194/gmd-2021-103
  code https://git.ufz.de/chs/MPR

We are aware of the MPR approach, but it is one of many regionalization methods that exist in the literature. So, it is not clear why this particular method would be specifically relevant. Moreover, MPR is based on a regional calibration framework while the present paper is not, so the methods are quite different.

- *MPR, in other words, uses the same ideas that the authors are proposing in this study (see table 1).*

We disagree, as mentioned above, as the methods are quite different.

- *MPR has been applied in many papers and projects (not an exhaustive list), but none rfered by the authors:*

1. *Samaniego et al. 2010: https://doi.org/10.1029/2008WR007695*
2. *Kumar et al. 2010: https://doi.org/10.1016/j.jhydrol.2010.07.047*
3. *Kumar et al. 2013: https://doi.org/10.1029/2012WR012195*
4. *Wohling et al 2013: https://link.springer.com/article/10.1007/s12665-013-2306-2*
5. *Livneh et al 2015: https://doi.org/10.1002/hyp.10601*
6. *Nijzink et al. 2016: doi:10.5194/hess-20-1151-2016*
7. *Zink et al. 2016 (German Drought Monitor) https://iopscience.iop.org/article/10.1088/1748-9326/11/7/074002*
8. *Rakovec et al. 2016: https://doi.org/10.1175/JHM-D-15-0054.1*
9. *Samaniego et al 2017: https://hess.copernicus.org/articles/21/4323/2017/hess-21-4323-2017.pdf*
10. *Demirel et al 2017: https://hess.copernicus.org/preprints/hess-2017-570/hess-2017-570.pdf*
11. *Zink el at https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2017WR021346*

12. Zink et al 2017 https://hess.copernicus.org/articles/21/1769/2017/

13. Mizukami et al. 2017 (MPR in VIC) https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2017WR020401

14. Samaniego et al 2018 https://www.nature.com/articles/s41558-018-0138-5

15. Demirel et al. 2018 https://hess.copernicus.org/articles/22/1299/2018/

16. Samaniego el al 2019: (C3S EDgE project)   https://journals.ametsoc.org/view/journals/bams/100/12/bams-d-17-0274.1.xml

17. Dembélé et al 2020:   https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2019WR026085

18. Lane et al. 2020 (in revision in WRR)

19. Guo et al. 2021 (MPR and other regionalization techniques apllied in VIC) https://journals.ametsoc.org/view/journals/bams/102/5/BAMS-D-20-0094.1.xml

20. Feigl et al 2021 https://link.springer.com/article/10.1007%2Fs00506-021-00766-

We appreciate the long list of the reviewer's own papers, but most of them refer to the MPR approach developed by the reviewer, and are not directly connected with the aims of the study. For example, Samaniego et al. (2018) and Zink et al. (2016) applied the MPR method for monitoring droughts which is not directly relevant to the present study. There is a potential link of Rakovec et al. (2016), Zink et al. (2017) and Dembélé et al (2020) with the present paper, and these papers will be referred to in the revised manuscript.

- *It is sad that all works on this subject are not even   mentioned. In my opinion, science is founded on previous knowledge. It could be that assumptions or parameterizations on these   studies is obsolete or not adequate. Here is where the authors should   provide hints on how to improve the state-of-the-art. Ignoring previous attempts is not a solution and not a good scientific practice.*

Many of the suggested references of the reviewer are cited in the review articles referred to in the manuscript and, besides, most of them are not directly relevant here. The purpose of the paper was not to improve the MPR and similar approaches but to test multiple-objective calibration in a regionalization context. We will, however, formulate more clearly in the discussion section that these approaches exist.

- *The regionalization of parameters of the equations 2 and 3 are similat to those proposed in Samaniego et al 2010a (WRR), and papers that follow.*

The hydrological model concepts are similar, but the regionalization of the model parameters is not. While Samaniego et al. (2010) disaggregate the infiltration model parameters calibrated at a coarse scale by using soil texture and land cover data, the present paper applies (in the regression based regionalization methods) an approach proposed by Merz et al. (2004, doi:10.1016/j.jhydrol.2003.09.028), where the model parameters in ungauged sites are estimated from linear regressions between calibrated model parameters in gauged catchments and the three most relevant catchment attributes related to topography, climate, river network, geology, land use, and soil types. In the revised manuscript we will make the origin of the regionalization methods used clearer, to avoid confusion with other methods.

Moreover, the regression-based regionalization is only one of the four groups of regionalization approaches tested in the present study, and the other groups are also different from the method of Samaniego et al. (2010).

- *In consequence of all these remarks, I can conclude that this study is not innovative, and hence not suitable for publication as a research article. The authors do not analyze the state of the are (e.g. MPR applied to HTESSEL or Noah Schweppe et al 2021) or very innovative approaches like Function Space Optimization (FSO) (Feigl et al 2021). The methods applied here are already common practice in operational hydrology.*

We beg to disagree. We consider the study innovative, as detailed above. While we do not analyze MPR, it was never the intention of the paper to do so. We believe the findings of the paper are novel and not already known.

- *I doubt that the ASCAT data can be used for SM evaluation. This product exhibits serious processing artifacts if a PCA is applied to the fields at large scale (This work was not published but presented at the EGU some years ago. I can provide the files if needed.) ASCAT, as far as I know is a passive signal and hence have a foot print that is too big to represent SM variability at the scale at which this process happens. In the best case it gets a signal from 2-5 cm depth. This is not what any land surface model can determine well due to many factors well ocumented in the literature. There are better technique nowadays to get SM at the plot scale with a passive method (CNRS Schrön et al WRR). From my own experience, ASCAT did not perform well in Germany or in Europe. For this reason we selected the blended ESA-CCI product (http://www.esa-soilmoisture- cci.org; Liu et al. 2011; Dorigo et al. 2014), which was the best performing product. ESA-CCI SM, however, ended up as the worse in the evaluation made by Rakovec et al. JHM 2016. Here anchor points with a footprint of the eddy covariance station was used. In the Study of Zink et al. HESS, 2017. (figure 6) a footprint of 100x100 m was used to verify the model (with regionalized parameters for whole Germany) against actual soil TDR/FDR moisture measurements. The model is able to reproduce the anomalies. Therefore, authors should compare against other SM products and methods to demonstrate that ASCAT is performing well in these particular sites. Dembélé et al 2020 also tested simulated SM against ESA-CCA and showed acceptable results.*

The reviewer seems to have some misconceptions about ASCAT soil moisture data. First of all, ASCAT is an active sensor, not passive as claimed by the reviewer, so much of the comments (in particular regarding resolution) do not apply. ASCAT is in fact one of the main datasets used in the blended product (ESA-CCI) recommended by the reviewer. Second, the reviewer's comment on the poor performance of ASCAT in Germany and Europe is not supported by any reference, such as in Luxembourg (Matgen et al., 2012, https://doi.org/10.1002/hyp.8316); Southwestern France (Albergel et al., 2009, https://doi.org/10.5194/hess-13-115-2009); and for selected networks across Europe (Brocca et al., 2011, https://doi.org/10.1016/j.rse.2011.08.003). Pfeil et al. (2018, https://doi.org/10.3390/rs10111788) and Hahn et al., (2017, https://doi.org/10.1109/JSTARS.2016.2628523) show that the 12.5 km ASCAT that uses the recent retrieval technology has improved accuracy with respect to previous soil moisture products. The reasonable to good performance found in these publications is already stated in the manuscript. One advantage of ASCAT is the very good temporal resolution over Europe. To address the spatial footprint, we use an experimental soil moisture product that downscales ASCAT with Sentinel data. To the best of our knowledge, this version of the ASCAT soil water index product with a spatial resolution 500 m is state of art. One of the aims of the recent study of Tong et al. (2021) was to test whether this dataset can improve the parameterization of a conceptual hydrologic model. Their results indicate that the ASCAT dataset performs very well, in particular in agricultural areas in a temperate climate. The performance for forests and in alpine terrain is somewhat lower, and some additional improvements are needed to correct for the effects of dense vegetation, snow-cover and frozen soils.