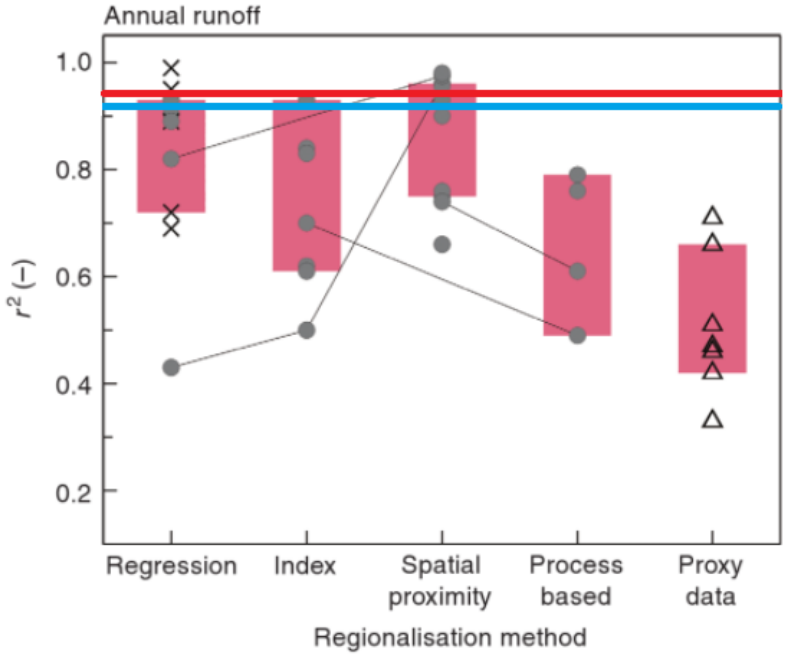
Detailed response to the comments of referee 2

We want to thank referee 2 for his review of our manuscript. In this author comment, we list how each of the remarks provided by the referee was addressed.

1. There are multiple possibilities to regionalize a distributed hydrological model and the present paper does not pretend to be exhaustive at all. On top of that, applying a single prescription method requires having some catchment characteristics for each model parameter which is absolutely not the case. Indeed, for the prescription method, each parameter of each hydrological mesh is prescribed according to the values of some catchment characteristics. However, MORDOR-TS is not a physically based model but a conceptual one. Regarding the only constraint method, it would require many proxy data which should be the subject of a whole article.

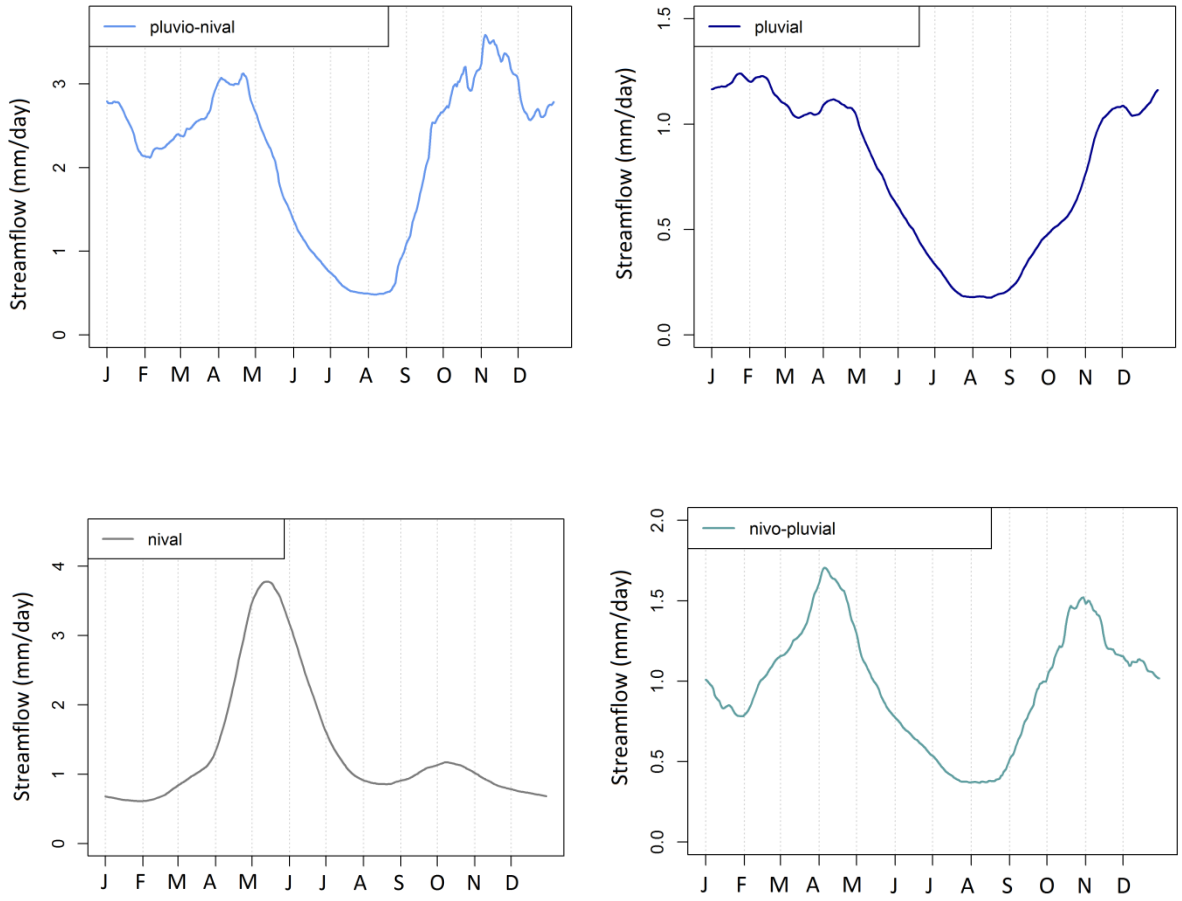
   ➔ We propose to add this sentence in the manuscript (Section 5):
   "The present paper does not claim to be exhaustive about the regionalisation methods. In fact, there are multiple possibilities to regionalise a distributed hydrological model with transposition, prescription and constraint. Here, we propose to compare a single transposition with a combination of the three regionalisation methods."

2. We agree that the improvements are not huge. However, prediction in ungauged basins is a very difficult issue which progresses very slowly. The figure below comes from Blöschl et al. (2013) and presents a synthesis of the literature about several regionalization methods for the estimation of annual runoff. We added to this figure two lines (the blue one for Exp1 and the red one for Exp2). According to this figure, our results are in the top part of the regionalization methods and the coefficient of determination rises from 0.92 for Exp1 to 0.94 for Exp2. So, in the context of prediction in ungauged basins, we think that the improvements we obtained with the combination of several regionalization methods are not marginal but significant.



Regarding the performance of Fig 10d, the performance actually declines between Exp1 and Exp2. However, hydrological modelling is always a compromise between several criteria. Here, we tried to have a convenient model for both daily runoff, seasonality, flood and low flow modelling. Nevertheless, if the aim was only to estimate low flows, the same analysis could have been conducted with a single objective function over low flows.

3. On this point, we do not agree with your remark. In our study, one basin (namely Loire or Durance) comprises hundreds of hydrological meshes with dozens of stations. In fact, the regionalisation of the Loire basin analyses 106 catchments while the regionalisation of the Durance basin analyses 34 catchments. Therefore, the number of catchments used in this paper is significant. On top of that, the regions are not climatologically homogeneous. With its 35 707 km², the Loire basin allows us to cover catchments from pluvio-nival to pluvial ; whereas the Durance basin with its 11 738 km² allows us to cover catchments from nival to nivo-pluvial. Therefore, the area studied within the scope of this paper is neither small nor climatologically homogeneous.



The philosophy of regionalisation developed in this paper aims to be generic and it could be applied to any hydrological models. In this study, this strategy is effectively tested for the only MORDOR-TS model. However, we tried to come out with general recommendations as described by Fig. 6. If the strategy described by this figure can be applied to any hydrological model, it necessarily requires a good understanding and a good knowledge of the model. But we do believe that a relevant regionalisation method can only be achieved if the model is well known, both physically and

numerically. However, testing the methodology over another hydrological model is out of the scope of this paper.

We agree with the idea of parcimony in hydrological modelling. However, the MORDOR-TS model has many parameters because it is intended to be applied to many climatic contexts. Indeed, it is used in the same time for nival basins and for pluvial basins, for glaciarized and for karstic basins, at hourly and daily timesteps. Some parameters were even introduced to compensate bias in precipitation or temperature estimations, to ensure good performances in every operational contexts. On top of that, Garavaglia et al. (2017) showed that having less parameters involves a significant loss of performance when tested on many basins.

4. We do not understand this comment. Can you reformulate it?

5. Concerning the terminology issue, also pointed out by referee #1, we tried to use short terms whose definitions are given at the beginning of the paper. On top of that, we tried as much as possible to use standard terminology, inspired by *Bloschl et al. (2013)*. However, our paper presents a new approach which is sometimes out of the scope of the existent literature. We did not find very satisfying alternatives, but we are open to any relevant suggestion.

References

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