Thanks to Referee #1 for the important comments given and we hope to satisfy all the concerns through this document.

CONCERN 1:

R: *My major concern lies in the synthetic nature of the study. SIR with parameter resampling is not new. Therefore, "proof of concept" via synthetic tests does not hold true in this case.*

A: We would like to state that we are not really performing a "Proof of concept" in this case, since the study addresses the benefits of using the SIR+PR methodology in order to increase the overall performance of the model. We are not focussing on the identification of model parameters. In this respect, it is important to state that the model makes use of a large number of parameters (more than 30 per grid cell). We have identified the parameters to which the soil moisture and discharge are most sensitive, and based on this knowledge we have developed a methodology that is relatively easy to apply and that does not require excessive computational power.

R: *Furthermore, this special issue focuses on real-world operations rather than pure research. Having said that, the paper as is might not interest the potential audience of this special issue.*

A: We think that this study is important for the potential audience who is interested in the development of new and easy-to-implement methodologies for the improvement of discharge predictions. Moreover, in this example-study the synthetic nature of the experiment allows us to demonstrate that, even in the case of a lack of soil moisture data for the bottom layers, and using a limited set of particles in the assimilation algorithm, it is possible to have appropriate baseflow corrections.

R: *To fit into the scope of this special issue, it is recommended that the authors add real-world experiments where real soil moisture observations are assimilated and the resulting discharge can be evaluated against real discharge observations (e.g., at the station in Pfaffenthal).*

A: We agree that it would be interesting to carry out an experiment with real data. Unfortunately, this is not possible due to limitations in real data sets for the Alzette catchment, more specifically with respect to remotely sensed soil moisture values. The objective of this study is to demonstrate a modification to a methodology (the particle filter) to assimilate data into a hydrologic model. We are working with a model that adequately represents the relationship between precipitation and discharge. It is thus unlikely that the conclusions of the study will change if real data are used instead of synthetic data.

R*: In that way, the "importance of parameter resampling" (as stated in the title) can be justified in a meaningful way (the current justification based on synthetic experiments is quite weak). Consequently, the impact of this paper will be considerably elevated.*

A: We believe that the application of the parameter resampling along with the particle filter can be justified here because of the overall improvement in the modelled baseflow. We demonstrate that the EnKF and the SIR filters can perform acceptably for soil moisture assimilation, but in order to have a positive impact on the other variables is important to resample the parameters. This is the main conclusion of the paper that we have also stated in the title.

CONCERN 2

R: *I am also concerned about the experimental design of this study. First, the inclusion of EnKF needs further justification. Apparently, EnKF and PF differ from each other in concept. Comparing the performance of two approaches is hardly fair. I would be cautious about this kind of* *comparison. As an example, the authors state that the SIR+PR serves as a solution to the shortcoming of EnKF performance (Page 5871, Lines 14-15). Note that EnKF has the capability to simultaneously update model states and parameters (and thus has the potential to overcome "the shortcoming" and even outperform the SIR+PR). However, if we have to compare both approaches, I would state that the EnKF is limited by its Gaussian assumption which likely has "negative effect" on baseflow.*

A: Due to similarity between this concern and the comment # 1 given by referee # 2, we kindly ask referee # 1 to read the answer posted to this comment. The paragraph below complements that answer.

We agree with the fact that, as is demonstrated in other papers referred to in this paper, both filters have the capability to estimate model parameters. In this study, instead of presenting a complete comparison between the filters when states and parameters are estimated, the parameter resampling is proposed as an effective and efficient (in terms of computational time) way to obtain improvements in the modelled baseflow. Therefore, we prefer to not compare the proposed methodology to the EnKF with parameter estimation. As stated above, the study does not focus on parameter identification, but on the development of a methodology to allow the particle filter to produce adequate model results through the assimilation of soil moisture values.

R: *Second, I would like to see more detailed explanation on the DACLM setting up. I believe that the model is configured with 4 grid cells and produces total discharge at the basin outlet, yet results from only one cell are shown. I am wondering whether four SM timeseries (for each of the four cells) or only one SM timeseries (for the cell of which results are illustrated) is assimilated.*

A: For each grid cell, one soil moisture time series is used, but this time series is different for all four cells. This will be specified in the model description section.

R: *I am also curious how assimilating SM in the top layer impact the SM in the remaining nine layers and subsequently the surface runoff, interflow, and baseflow. I am suspicious that this impact would be more in the sense of statistics instead of physical dynamics, given the weak physical correlation between top layer SM and SM in bottom layers (e.g., the 100 cm depth).*

A: This is related to the SIR+PR methodology and will be extended in the document. The resampling index (refered in section 5.2.2) is obtained based on the observations corresponding to the top soil layer and used in the state updating. The same resampling index is applied to the remaining nine layers and to the parameter set. As a consequence, the ten soil layers are highly correlated leading to a significant improvement of the baseflow.

R: *I have some further concerns on the results presented. First, Fig.5 and Figs. 7 – 10 show the SM comparisons. Given the fact that there are only SM (synthetic) observa tions in the top layer, I guess the SM shown in these figures is exclusively for the top layer (other than the total SM from 10 layers). Looking at Figs. 9 and 10, after Feb.10, 2007, the SIR+PR derived baseflow is almost identical to the synthetic truth, which is mathematically astonishing. However, physically, it is hardly possible that assimilating only top layer SM would lead to "perfect" baseflow simulations, since top layer SM is more correlated to surface runoff or interflow rather than baseflow which relates more to SM in bottom layers. As an example, in Figs. 7-10 when no SM is assimilated (i.e., from Jan. 1 to Feb. 8), model-simulated SM is fairly close to the synthetic true SM. However, the discrepancy between model-simulated baseflow and synthetic true baseflow is significant, indicating that the baseflow is not dominated by top layer SM. These observations raise the skepticism that the SIR+PR might violate the physical relationship between SM and baseflow through mathematical intervention (i.e., adjusting model parameters in a disconnected way), which* *is more evident when comparing to Figs. 7 and 8 (which show that assimilating top layer SM via EnKF and SIR actually can't improve the baseflow simulation much, while EnKF and SIR preserve the model physics but not introducing inconsistent parameters in different measurement intervals). Yet it might be arguable that assimilating top layer SM would improve estimates on SM in other layers, thus resulting in improved baseflow estimates. However, whether (and how much) assimilation of top layer SM would improve SM in other layers needs to be justified, which goes back to my second point in my concerns on the experimental design.*

A: We consider that the synthetic study is reliable, because before carrying out the assimilation experiment the hydrologic model was calibrated for discharge observations. Therefore, the model is a good approximation of the reality concerning the relationship between precipitation, soil moisture and baseflow/discharge. The excellent match between the synthetic truth and the model results can be explained by the synthetic nature of the study. However, since we are working with a well-calibrated model, the conclusions can be expected to not alter if real data would be used.

R: *Second, Table 4 shows that SIR+PR derived SM deteriorates in accuracy with decreasing observation frequency (e.g., from every week to every four weeks). In contrast, Table 5 shows that SIR+PR derived baseflow improves in accuracy with decreasing observation frequency. Does this imply that the less SM is assimilated, the more accurate the baseflow estimates? And in Figs. 9 and 10, if only 4 DA events (rather than 16) are considered, the baseflow simulation will further better mimic the synthetic truth?*

A: Although the baseflow RMSE indices indicate an improvement when decreasing the observation frequency, we consider that the difference in the order of magnitude is small between the three values. If we present these results graphically as in Fig. 10, we would not be able to notice any difference since the difference is small.

MINOR COMMENTS:

R: *Lastly, I have some minor comments: a) In Abstract, Results, and Conclusions sections, the usage of "discharge" is confusing. I guess the authors mean baseflow specifically. Note in Line 6 (Page 5854), "discharge" represents total outflow at the gauge.*

A: Thanks for this comment, we will correct discharge by baseflow in the document.

R: *b) The paragraph in Lines 11-22 in Page 5856 seems to be unrelated to the work presented in the study and thus redundant.*

A: We will rephrase the lines of the paragraph in order to make them more convincing.

R: *c) The sentence in Lines 16-18 in Page 5857. The reason is invalid in that I) direct relationship is not the equivalent of linear relationship; II) there are other states like temperature which does not "correspond directly" to soil moisture in an explicit way.*

A: We will correct this.

R: *d) "Negative (positive) effect" or "negatively (positively) affect" appear numerous times in the context. Please be specified (e.g., overestimate, underestimate) since "negative" (or "positive") is more a relative concept and thus not clear in meaning sometimes.*

A: The terms positive/negative will be changed.

R: *e) When calculating RMSE for baseflow (in Figs. 7 – 10), an alternative period should be used (i.e., starting from Feb. 8 rather than Jan.1), since the model is not warmed up enough in the* *beginning and errors in initial conditions largely impact baseflow simulations. Additionally, DA is not applied prior to Feb. 8, that period (Jan.1-Feb.8) contains no information regarding the performance of DA. Inclusion of that period in calculating the metrics (e.g.,RMSE) dilutes the actual performance of DA. As can be told from Figs. 9 and 10, the RMSE of baseflow is largely from this period (while the DA is only applied in the remaining period).*

A: In page 5866 lines 21-23 we indicate the time period related to the RMSE index computation when DA is performed. We will rephrase the sentence to make it more clear.