The authors have revised and reformulated their manuscript in large parts. This did increase the quality. However, I cannot understand why this was not done before submission. Theses late changes increase the work for the reviewers.

On top of this, the new manuscript partly revokes at least one change from the previous version.
I had asked before:
Page 11 Lines 34-36: "The soil moisture observation error is assumed to be normally distributed with mean equal to 0 and standard deviation equal to 0.02m^3/m^3, for both VIC-3L and CLM." Please discuss why you assume this uncertainty, especially since it is a mean of 41 values.
To which the authors answered:
Reply: We admit that 0.02m^{3}3/m^3 is a little larger than the uncertainty of the mean soil moisture content averaged over the 41 values. A larger observation error elevates potential problems with filter inbreeding. In addition, it adds flexibility in case of the presence of an observation bias or model structural error. This was added in the revised version of the manuscript (line 448-451):
"We admit that 0.02m^3/m^3 is a little larger than the uncertainty of the mean soil moisture content averaged over the 41 values. A larger observation error elevates potential problems with filter inbreeding. In addition, it adds flexibility in case of the presence of an observation bias or model structural error."
Now it reads (Page 16, Lines 573-576): "The measurement errors of the soil moisture observations are assumed to be zero-mean Gaussian with standard deviation, $\sigma = 0.02$ m^3/m^3. This results in $\mathbf{R} = 4 \cdot 10^{-4}\mathbf{I}_m$ in equations (4) and (17), respectively. The value of σ is set larger than its default of say 0.01 m^3/m^3 to compensate, at least in part, for the lack of use of an explicit error model."
I might be a little pedantic about this and the chosen error of 0.02 m^3/m^3 seems appropriate, as explained by the authors. But, the error of the mean of 41 measurements with individual errors of 0.02 m^3/m^3 is 0.003 m^3/m^3. I think it is a different message if the measurement uncertainty was increased by a factor of 2 or by a factor of over 6. Please correct this.
Due to the rewriting of almost the complete manuscript, these changes go unnoticed (and at least in this instance have also altered the given statement). Hence, I ask the authors to answer all the previously asked questions, that resulted in changes of the text, again.

I have also comments regarding two previous questions, that were not answered sufficient to me:

Regarding my previous question 4: I had asked, what was changed to achieve the different results for the VIC-3L parameter b with MCMC (in the first manuscript the value had deviated strongly from the results by the other assimilation methods).
The authors answered: "We redid all the experiments including the generation of the initial ensemble of parameters."
What exactly did you change in the generation of the initial ensemble of parameters? Was it e.g. just the seed for the generation of random numbers? If this is the case, I think it is an important information, that the seed had to be tuned to achieve a consistent result with the other methods and should be mentioned in the manuscript. Please clarify this.

Regarding my previous question 2: I had asked about the impact of different initial parameter uncertainties (in the new manuscript called *prior parameter distribution*) for the estimation with the EnKF in the context of the chosen inflation method. The authors answer "[...] that this uncertainty was not underestimated [...]" and refer to the paper by Whitaker and Hamill (2012).
However, the authors have chosen a special case of the inflation method proposed by Whitaker and Hamill (2012): They keep the parameter spread constant, while Whitaker and Hamill (2012) actually scale the relaxation to the prior spread with a tuning parameter. In their case the best results were not obtained with

the special case of keeping the spread constant. In my understanding, this choice additionally leads to a direct permanent coupling of the parameter uncertainty at each time step to the initially chosen uncertainty. Due to this introduced coupling, I think, that the choice of the initial parameter uncertainty (*prior parameter distribution*) is important. Thus, it might be not enough to ensure that the initial parameter uncertainty is not underestimated. It is possible that the uncertainty was chosen too large and that a smaller uncertainty could actually improve the results.
Please discuss this or explain where I'm wrong.