General Comments
The authors addressed most of the reviewers' comments and, in my opinion, the paper was significantly improved. Specifically, a more detailed description on the LISFLOOD model calibration and on the data assimilation procedure was added in the paper. However, the reading of the paper submitted on WRR by Wanders et al. (2014) is still needed to fully understand the calibration procedure. It would have been easier if the authors had provided this paper as auxiliary material.
The authors agree that it would be more convenient if the WRR paper would be accessible to the reader. Unfortunately the authors are unable to include the WRR paper as auxiliary material. However, the authors believe that after the corrections made in the revised manuscript, the paper can be read without the need for the WRR paper.

Moreover, I still have two major comments/suggestions that, in my opinion, should be addressed before the publication.

1) The soil moisture timeseries shown in Figure 10 are for me not sufficient to have an idea of the agreement between satellite and modelled data and, hence, their impact on the model. The visualized timeseries are too short and looking this figure is not clear how it is possible that satellite soil moisture data could provide some improvements. Can the author show a longer time series? What is the correlation between modelled and satellite data? In the figure, the model simulation without the assimilation should be also shown (e.g. the ensemble mean). In my opinion, this will clarify the reader to see the potential of satellite data for hydrological modelling.

We kindly thank the reviewer for the suggested improvements. As proposed by the reviewer the timeseries have been extended to include the entire summer period. A longer timeperiod will reduce the readability of the figure due to the high number of satellite observations. The correlations have been omitted since the focus of this paper is on the application of the soil moisture observations and the authors wish to focus on this topic.
The uncorrected soil moisture has been included in the Figure to have a better assessment of the improvements made by remotely sensed soil moisture. The new Figure has been included in the revised manuscript.

2) I like that the authors added the simulation with the assimilation of ONLY satellite soil moisture data and in the configuration for which the model was calibrated with 7 discharge stations. This configuration could represent the real operational conditions of the EFAS system and, hence, the assimilation experiments carried out in the study really show that the assimilation could improve the EFAS system. However, I would give a slightly different interpretation to the results shown in Table 2 (if I understood correctly!). In my opinion, the results should be analysed in the case of no assimilation (baseline configuration, Q7noDA) and compared with those considering the assimilation of only soil moisture (Q7satDA), only discharge (Q7), and soil moisture plus discharge (Q7sat). By doing this analysis (see the figure below), it appears evident that the assimilation of soil moisture or discharge (only) provides only a little improvement in the performance while the joint assimilation has a significant positive impact. This is not expected to me as I suppose that the impact of assimilating discharge should be higher than soil moisture, and I do not expect the large differences that are obtained from the joint assimilation. Do the authors have some explanations for that?

According to the reviewers suggestion section 3.4 has been modified to more clear on the results with limited assimilation. The section has been extended with a general remark on the specific impact of both satellite and discharge assimilation in terms of their impact on

the simulation discharge. The authors believe that the joint assimilation results in big difference due to the fact that the intermediate flow regimes are largely impacted by the assimilation of the soil moisture, while the more extreme events benefit more from assimilation of discharge. Additionally, assimilation of soil moisture results in a strong reduction of the uncertainty in simulated discharge. If this reduction in uncertainty is combined with a better estimate of the level of the flood than this will result in strongly increased flood forecasting performance. This hypothesis is supported by the fact that the BS Q80 strongly reduced with assimilation of only satellite data (Q7satDA) compared to only discharge observations (Q7). Additionally, the CRPS is also strongly reduced leading to an overall more accurate estimation of discharge level with small uncertainty. For the extreme event BS Q90 the impact of satellite data is negligible. However, in these situations the discharge assimilation has a higher impact as is the case for the cv which is strongly reduced by discharge assimilation.
However, the authors acknowledge that more research is required on the exact impact of the individual compenent in different large-scale catchments and with different models.

Specific Comments

P2, L11: "ensure optimal performance". I suggest smoothing this sentence as the true errors of satellite soil moisture products are unknown and the optimal performance cannot be ensured.
We modified the sentence to ensure increased performance

P2, L15: "reduced by 65%". To which comparison are you referring? I was not able to understand from the results reported in Table 2.
The reviewer is correct, this number is incorrect and is modified to 35% which is the difference between the performance of Q1 and Q7

P5, L15: Results with the assimilation of soil moisture only are also shown in the revised manuscript. Please revise.
The sentences has been revised to include the new scenarios that where included in the paper after revision.

P6, L16: It should be 2 layers for the simulation of the unsaturated zone in the original version of LISFLOOD.
The manuscript has been modified accordingly

P7, L12: Typo "mount" should be "amount"
The manuscript has been modified accordingly

P12, L2: Can the authors specify the measurement error covariance for soil moisture observations? At least the mean values could be reported. This would be particularly important for future studies aiming to assimilate these satellite products.
The average obtained errors of the Wanders et al. 2012 paper have been included in the revised manuscript. This will allow the reader to understand the paper without Wanders et al. 2012 as correctly suggested by the reviewer.

P13, L28: I would add the word “realizations” after “102 and 15300”
The manuscript has been modified accordingly

P14, L3: Typo “comclided” should be “concluded”, I guess.
The reviewer is correct and the line has been adjusted

P17, L15: Specify better that the overestimation is present in the period selected for the visualization in Figure 4 that does not represent the whole period. I needed to reread the paper more times to understand this.
We added a line to clarify this statement. The new text is: *However, on average the bias does not exist for the entire simulation period and no systematic bias exist between the simulation and the observation.*

P17, L28-29: Please revise this sentence, as it is not clear.
The manuscript has been modified to make a better and more clear statement.

P19, L24: Section 3.4 – This section is not clear to me, see the General Comments. For instance, it reads: “The uncertainty in …. is reduced for Q7noDA compared to Q7”. From the results shown in Table 2 it seems to be the opposite, please check.

The reviewer is correct this statement is incorrect. We modified to text to be consistent.