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Discussions

Interactive comment on "Observation operators for assimilation of satellite observations in fluvial inundation forecasting" *by* Elizabeth S. Cooper et al.

Anonymous Referee #1

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The manuscript "Observation operators for assimilation of satellite observations in fluvial inundation forecasting" by Cooper et al. aims at improving the methodology for assimilating SAR-derived observations into hydrodynamic models for flood forecast. More specifically, the study investigates the use of three observation operators, which are (1) water level observations at flood edge position, (2) water level observations at the nearest wet pixels, and (3) backscatter measurements. Use (1) and (2) has been previously investigated; the proposed analysis focuses on the use of (3) within an ensemble Kalman filtering approach. Synthetic twin experiments are used to investigate the physical mechanisms by which different observation operators update modelled water levels; the performances of these operators are analysed to compare

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Discussion paper

the strengths and weaknesses of the three different approaches. The non-linear hydrodynamic model Clawpack is used simulate the propagation of a 110 hours flood hydrograph in an idealised domain. A model realization is considered as truth and it is used to produce synthetic SAR observations. Uncertainties in the input hydrograph and in the assessment of the channel roughness are considered in this study. The ensemble Kalman filtering framework is used to assimilate (1), (2), or (3) within a state updating and a state and parameters updating approach. Assimilations are performed at 5 transects and every 12 hours. The synthetic experiments confirmed the conclusions of previous studies on the limitations of the observation operator (1). This manuscript provides a detailed description of the physical mechanisms leading to the highlighted limitations. Observation operators (2) and (3) were both effective, with (2) allowing quicker retrieval of the truth values but having problems in in-bank flow conditions. Backscatter observation operator (3) also allowed the forecast to converge to the true solution for both water levels and channel friction parameter value in the joint state-parameter experiments.

In my opinion, this study is interesting and provides a valuable contribution to the literature. The manuscript is well written, the introduction is explicative and provides a comprehensive overview of the problem, the methodology is detailed, the results are presented in an effective manner, the discussion provides a thorough analysis, and the conclusions provide a clear summary of the main findings of the study.

I would like to recommend the publication of this manuscript after minor revisions.

More specifically, I would like to suggest some clarification on the methodology and a further discussion on the potential practical use of the observation operator (3).

Firstly, I would like to recommend rewording the sentence in lines 4-9 page 14. The spatial distribution of the backscatter observations is a crucial aspect of this study and I think that this sentence is a bit confusing. The authors might (or might not) consider adding a graphic explanation (maybe by adding details to one of the figures of the manuscript).

Secondly, I understand that the backscatter observation operator (3) can reduce the computational time and it has the potential to enable the assimilation of a larger number of data. In fact, (3) can be used when a DEM is not available thus allowing SAR data assimilation in a larger number of case studies. Moreover, (3) can enable the assimilation of a larger number of points within the same case study. Do the authors think that the merits of observation operator (3) are due to this "increased data availability" and/or to the different physical mechanism underpinning the assimilation of backscatter rather than SAR-derived water level values? One thing that I could not understand from the manuscript is whether the methodology implied that a pixel with backscatter lower than the mode mw is more likely to be flooded than a pixel having backscatter mw.

As the authors underlined, the number and spatial distribution of backscatter observations used in the data assimilation approach has to be carefully defined. I think that this is a critical aspect for the practical application of the methodology. SAR-derived inundation maps are affected by a large number of uncertainties. For instance, the histogram analysis used here might not be a reliable approach in catchments with emerging flooded vegetation where double bounce effects are not limited to the flood edge. The flood edge itself is often the area of largest uncertainty. Furthermore, SAR-derived inundation maps computed using histogram thresholding are often fragmented and further analysis steps such as region growing or use of ancillary data are required to produce a "continuous" inundation layer. These steps are not included in the methodology presented in the manuscript. Would the authors recommend adding these (or similar) steps in a real case scenario? Pixels within flooded areas might have large backscatter due to double bouncing effects, speckle, and other uncertainties. If such pixels are used as backscatter observations, the data assimilation approach will degrade the performance of the flood forecasting model. Is this a possible scenario? If so, how do the authors recommend avoiding this problem?

I understand that this paper focused on a synthetic experiment and I agree with the authors that a detailed investigation of physical mechanisms within a simplified context is essential to develop knowledge and to explore the feasibility of proposed techniques. However, my main comment concerns the effectiveness of the proposed method in a real world scenario. I think that a further discussion on the potential hurdles and possible solutions for the implementation of observation operator (3) in a real case study would facilitate the reception of the proposed approach and encourage its application. The authors might consider adding a short description of the overall characteristics of the real world scenarios for which they think that their method could provide reliable results. An overall description of the characteristics of the real world scenarios for which the backscatter observation operator is not recommended could also provide useful information to the readers.

Finally, I listed below a few detailed remarks.

I hope the authors will find my comments and questions useful to improve their manuscript.

Page 9, line 21: I think the second parenthesis after George (2018) should be removed.

Page 10, line 18: a full stop should be added before “Inflows”.

Page 11, line 19: “ch” in the symbol for channel roughness should be a subscript.

Page 12, line 3: are the intervals of roughness values correct?

Page 13, Figure 3: are the values in the x-axis correct?

Page 13, Figure 3: low backscatter values are usually represented with dark grey to black, high backscatter values are usually represented with light grey to white. In the colour scale used in this figure, the higher the backscatter value, the darker the pixel. Despite this is just a cosmetic detail, I was wondering whether the authors are willing to reverse the current colour scale to allow a more straightforward interpretation of the figure.

Page 15, line 12: “growns” should be corrected.

Page 18, Figure 7c: the authors might consider adding a description of the red arrow.

Page 19, line 12: the authors might consider rewording the sentence “the water level predicted by the observation at the observation location” to improve the readability of the paragraph. More specifically, “predicted” might not be the most appropriate word in this context.

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