

Interactive comment on “Calibration of channel depth and friction parameters in the LISFLOOD-FP hydraulic model using medium resolution SAR data” by M. Wood et al.

Anonymous Referee #1

Received and published: 15 February 2016

Review of the paper:

Calibration of channel depth and friction parameters in the LISFLOOD-FP hydraulic model using medium resolution SAR data By M. Wood, R. Hostache, J. Neal, T. Wagener, L. Giustarini, M. Chini, G. Corato, P. Matgen & P. Bates

The paper presents a method to calibrate simultaneously the bankfull channel depth and channel roughness parameters within a 2D LISFLOOD-FP hydraulic model using an archive of moderate (75m) resolution SAR satellite-derived flood extent maps and a binary performance measure for a 30x50km domain covering the confluence of the rivers Severn and Avon in the UK. The unknown channel parameters are located by a novel technique utilizing the Information Content and identifiability, (previously devel-

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oped by one of the authors of the manuscript) of the single and combinations of SAR flood extent maps to find the optimum images for model calibration.

GENERAL COMMENTS

Overall, I found the paper very interesting and adequate for the publication in the HESS journal. The importance of the understanding of the value of single or multiple SAR images in the identification of hydraulic model parameters is very important since – due the ever greater loss of ground information – the use of satellite images will be increasing in the near future. However, I found many drawbacks mainly due to the paper organization and presentation that have to be solved by the authors before it can be considered adequate for a publication in the HESS journal. Moreover there are some key points that I would like to underline.

One is the justification of the small sensitivity of the roughness parameter with respect to the channel bathymetry that to me seems reasonable but I found some difficulty in understanding if this is supported by a robust analysis or it is not adequately shown in the manuscript. To this end, it would be interesting to see the DNYA analysis carried out also for the channel roughness in order to recognize its information content and its value of identifiability. If this was already done, but not shown, some comments or an explaining figure would be very welcome.

A second point is the assumption that the error related to the processing of the SAR image will not affect the results (not considered for simplicity). I think that these errors are part of the procedure of identifiability and are able to affect the information content of the different images. For this point my question is: due to the different acquisition, times of the SAR images under different atmospheric and land conditions can be considered the error related to the image processing stationary? My opinion is that this error varies from image to image. This at least deserves some discussion. The authors could consider that aerial flood maps for analyzing this point or, since the area is very well instrumented, doing the same type of analysis with stage data.

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A final point is the quality of all the figures in the manuscript that I found very poor and such that to impede a proper understanding of the manuscript.

Based on that I recommend publication after major revisions. In the following, the authors can find a list of comments with the associated relevance listed in order of appearance in the manuscript.

COMMENTS

Pag. 2 Lines 53-76 MINOR: the authors may also cite the work of Moramarco et al. (2013) which uses an interesting method for identifying the flow depth distribution in natural channels.

Moramarco, T., Corato, G., Melone, F., Singh, V.P., 2013. An entropy-based method for determining the flow depth distribution in natural channels. Journal of Hydrology, 497,176-188.

Pag. 3 Lines 88-91 MINOR: Can you rephrase this sentence more clearly?

Pag. 4 Lines 120-128 MAJOR: If I understand correctly the channel depth is expressed as $H=r*B$ where H is the channel depth, and B is the width of the channel. Since the hypothesis of linear scaling is central in the study, I think this part deserves more profound discussion about: 1) How much it will affect the results of the study. 2) Which are the expected problematics associated with the uniform channel depth.

See also the paper of Yan et al. (2014) where H is a free parameter of the model uniform along the river reach.

Yan, Kun, et al. "Exploring the potential of SRTM topography and radar altimetry to support flood propagation modeling: Danube case study." Journal of Hydrologic Engineering 20.2 (2014): 04014048.

Pag 3 Section 1.2. MINOR: A scheme or figure of the method would significantly help to understand the image-processing algorithm.

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Pag. 6 Lines 211-213: MODERATE: it is not clear how the procedure is used with multiple images. Please provide more details.

Pag 7 Figure 1 MINOR: The quality of this figure is very poor. Please provide a larger and cleared picture where the identification of the study area and the boundary conditions are more clearly visible.

Pag 11 Figure 2 MODERATE: the quality and the description of this figure is very poor. Also, ENVISAT and Aerial data seem to be a bit different although with this picture is very difficult to compare the results. I understand that the processing of the SAR image inherently contain errors, I am wondering if the results of the paper might be affected by these errors. The authors could test the procedure also on the aerial photograph to understand the effect of the errors in the processing of the image or on the observed stages.

The observed model which is expected to behave better than the test model seems to be worse than the test model? Do you have a justification for that? Does this depend on the calibration?

Pag 11 Figure 3 MAJOR: Please provide a better figure with colors. It is very difficult (with this figure) to follow the authors' statements.

Pag 11 Section 3.1 MODERATE: I found this section very difficult to follow and to read. I suggest to try to present it better.

Pag 12 lines 346-349, MAJOR: the authors concluded that n_c is insensitive when estimated simultaneously with the channel depth. However, it seems that this was concluded based only on two images (23rd July 10:27, and 17 January 2008 21:55). Do the authors exclude that this is true in any case and there are not effects of the time of acquisition and the magnitude of the flood event? I think the authors should provide more proofs for this statement. Overall, I find this assumption reasonable however I think that including the DYNIA also for the parameter n_c would add a lot of value to the

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paper.

Pag. 13 line 361. MODERATE: It is not clear how the IC score is calculated for multiple images.

Figures 4,5, 6, 7 and 8 MAJOR: The interpretation of these figures must be described in the method section. From the text it is very difficult to follow the authors' statements. Please provide a better quality figures as well. It is impossible to discriminate between the different lines. If I understand correctly these are the cumulative distribution of the rescaled support values and not the gradient. The gradient should refer to their slopes. Isn't? If so, I expect a figure like the one in the paper of Wagener et al. (2003), (see FIGURE 8 in their paper).

Pag 18 lines 503. MODERATE: No colors can be seen in the figures.

Pag 19 lines 545-556. MODERATE: I expect here some discussions about the possible consequences of the assumptions made in the paper.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2015-511, 2016.

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