Hydrol. Earth Syst. Sci. Discuss., 5, S2079–S2085, 2008
www.hydrol-earth-syst-sci-discuss.net/5/S2079/2008/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Improved estimation of flood parameters by combining space based SAR data with very high resolution digital elevation data" *by* H. Zwenzner and S. Voigt

Anonymous Referee #1

Received and published: 3 December 2008

This paper presents a methodology for extracting reliable water stages from satellite images of floods via a combination of space-based SAR data with high accuracy DEMs. The methodology aims at increasing the geometric and thematic accuracy of remote sensing-derived water stages. Although a rather similar approach was presented by Schumann et al. (2006), the approach that is shown here provides some new elements that appear quite innovative (e.g. the extending and trimming of the flood mask to increase the coherence with the underlying DEM).

One concern I have with this paper is that, in my opinion, the methodological approach

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

is not sufficiently well explained and that part of the argumentation appears to be illogic. Many processing steps remain very vague and unclear (e.g. 'certain segments with a mean elevation above a certain elevation are excluded'). The key sentence of this paper ('Differences are balanced...') leaves room to several interpretations. What do you mean by 'the lowest flood profile elevation'? Is this the mean elevation extracted at the left and right edge of each profile segment? Would the result be the same if you would minimise the differences between water stages extracted on the two edges of each profile segment? I would ask the authors of this paper to clarify the methodology in order to avoid any misinterpretation. What appears to be rather illogic in the methodological approach is that in the first processing step the authors have to assume that there are no classification errors, because if there are classification errors, there is no point in shifting the flood segment. By following this procedure, the relocation of flood segments that are influenced by other errors than positional errors will lead to an over- or underestimation of the true water levels. By trimming and extending the flooded segments once they are repositioned, the method merely helps to make sure that there is a coherence between RS-derived flood extents and topography (the elevation will remain the same). Hence I don’t agree with the authors that this second processing step enables the correction of classification errors. It might be possible in case of minor thematic errors (in the order of some pixels) but it is certainly not true for larger areas that were misclassified (especially flooded areas that were considered as non-flooded). Unfortunately the authors don't discuss this issue and the data that was available for the two case studies doesn't allow them to demonstrate that they are right. Imagine a flooded vegetated area. The flooded area that would be visible on the SAR image would end at the edge of the vegetated area. With the methodology that is presented in this paper one would shift the flood segment to a position that will give a too low profile elevation. Consequently, the adjusted water stage would considerably underestimate the true water stage. Actually this might have happened in the case study of Tewkesbury. In order to circumvent this problem I would suggest applying a mask that excludes all urban settlements and vegetated areas from the analysis.

HESSD

5, S2079–S2085, 2008

Interactive Comment

In conclusion, I am rather sceptical about this methodology's capability to correct for classification errors and I would even say that the processing steps might cause gross errors if areas with classification errors are considered. I would appreciate if the authors would develop a discussion around some of the limitations of this method and I would recommend them not to claim that the method can correct thematic errors. It is just an assumption that is not proven in this paper (see comment below). I agree with the authors that their method allows to achieve a coherence with the topographic data (which is a good thing)

One of the main differences between this method and the one given by Schumann et al. (2006) is related to the choice of the authors to independently shift the cross sections. Whereas Schumann et al. (2006) applied a same dx,dy shift to the entire flood mask, here the authors look for a different 'optimal' shift for each flood segment. I would be very interested in knowing if the same (or at least similar) dx,dy couple (inferred from the shift along the cross section) was found for every section (which would speak in favour of a systematic geocoding error) or if rather different values were found. If one could see how the dy,dy values are distributed along the reach one could probably distinguish between positional and thematic errors.

Another rather annoying thing about this paper is that there is no means to verify the reliability and plausibility of the methodology. In fact, in both case studies, the reference data is provided by two optical images that were acquired several hours before or after the SAR acquisition. Without being able to see how the situation evolved between the two acquisitions, the comparison is pretty much useless. I would appreciate if the authors would provide the flood hydrographs that are depicting the timing of the two acquisitions. Also, for a real validation it would be necessary to have some ground truth data with respect to water stages. The validity of the methodology cannot be verified by the means of flood extent imagery alone. The authors claim that their approach can correct thematic errors. With the data at hand they cannot proof their a priori assumption (especially in the light of the serious doubts that I rose in the previous

HESSD

5, S2079–S2085, 2008

section).

Specific comments:

p. 2955 l. 2 the underlying assumption is that the water stage is horizontal along a given profile. The profiles need to be chosen in such a way that this assumption remains valid

p. 2955 l. 15 very vague! Please explain in more detail how you choose these thresholds. I assume that this threshold changes in a downward direction along the river flow line because you cannot choose a single mean elevation for the entire reach

p. 2955 l. 21 what is the 'lowest flood profile elevation'? How do you compute it? (average between the elevations extracted at the left and right border of each segment?)

p. 2955 l. 21 I am not sure about the term 'moving window'? Can you develop the explanation of this processing step?

p. 2955 l. 28 this processing step was not applied in the Elbe case study. Why?

p 2956 l. 3 I don't agree (see comments above), I agree though that this step ensures a coherence with the underlying DEM that might in some cases compensate minor classification errors.

p. 2956 l. 7 - l. 10) this is a preliminary discussion/conclusion

p. 2956 l. 22 'gradient is about 7 m'. Meaning?

p 2956 l. 2 how did you geocode the image? What is the accuracy with respect to ground control points?

p. 2957 l. 5 does this filtering step over 7x7 pixels preserve the edge of the flood boundary?

p. 2957 l. 10 rather confusing sentence! What reference data was used to guide your trial an error procedure? How did you evaluate the errors?

HESSD

5, S2079–S2085, 2008

p. 2957 l. 18 contradiction with Fig. 1: 3 or 5 hours?

p. 2958 l. 1 why Fig.4 before Fig. 2?

p. 2958 l. 11 please comment the differences that are shown on Fig. 3 , also I would suggest to maintain the same axes Why did you not apply the moving average?

p. 2958 l. 17 nothing is proven here! This is a mere visual analysis. After the processing the water surface line appears to be more plausible than before but the 'hydraulical plausibility' cannot be proven with the data at hand. In fact the lines are still not hydraulically plausible (the data points are still scattered)!

p. 2958 l. 17 This is not a confirmation! At best it is another indication! 'normal water level' ?

p. 2958 l. 25 in this case the shifting method doesn't give reasonable results. Please comment. p. 2960 l. 3 “ cannot be guaranteed” This is a nice euphemism! If you consider IKONOS as a valuable reference, a 2m difference cannot be tolerated. This would prove that the method failed. How do you explain this systematic underestimation of the water surface line.

p. 2959 l.9 'previous' instead of 'last highest'

p.2959 l. 11 - 13 this seems to be a contradiction: 'the flood situation was stable with two local maxima'. Can you show the hydrograph?

p. 2960 l. 2 what is the multiresolution segmentation?

p. 2960 how did you geocode the TSX image? What is the accuracy with respect to ground control points?

p. 2960 l. 17 does this mean that the shift that is applied is always an integer number? Why did you not apply floating numbers? I mean that the geometric error probably doesn't correspond to an integer number.

HESSD

5, S2079–S2085, 2008

p. 2963 l. 17 is it not possible to detect flooding inside urban areas with TSX data?

p. 2961 l. 26 in my opinion a likely reason could be that your approach didn't allow to detect the flooding within the urban settlement. Hence you relocate the flooded segment to a place where the intersection with the underlying DEM provides water stages that underestimate the true water stages.

p. 2962 l. 11 delete 'Discussion of Results and Conclusions'

p. 2963 l. 5 not only with the R-1 image! The results obtained with TSX are similarly disappointing (cf. Fig. 6 am Fig. 7)

p. 2963 l. 8.- l. 13 This paragraph is overly optimistic. Since there was apparently no data available that would allow a true validation, I can't see how you can conclude this. How can you claim the 'methodology allows the generation of reliable and hydraulically sound maps'?? When I look at the Elbe data, there is obviously a strong disagreement with respect to the waters stages derived from Ikonos imagery (assuming the latter to be a valid reference - which it probably isn&'t). The case study in the UK also shows that the water stages derived from TSX disagree with those obtained from aerial photography (Fig.7). Moreover, the water surface line that is depicted in Fig. 8 can't be considered as 'hydraulically sound' ! Please change the wording in this paragraph because in my opinion it gives the impression that you gloss over the results that were shown in this paper.

Fig. 1: 5 hours or 3 hours? Fig.2 meaning of 'mean water level' ? Fig.3 meaning of 'normal water level&' ? Fig. 4 meaning of second sentence in the legend is rather unclear to me Fig. 6 meaning of 'mean water level' ?

Reference:

Schumann, G., Black, A., Cutler, M., Henry, J.-B., Hoffmann, L., Matgen, P., & Pfister, L. 2006. Hydraulic and event knowledge to reduce the positional uncertainty in SAR flood images for improved flood model calibration and development, In Proceedings

HESSD

5, S2079–S2085, 2008

of Accuracy 2006, 7th International Symposium on Spatial Accuracy Assessment in Natural Resources and Environmental Sciences, Lisboa, Portugal, 5-8 July 2006, pp. 633-642

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 2951, 2008.