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Hydrology and Earth System Sciences Discussions

Interactive comment on "Use of ENVISAT ASAR Global Monitoring Mode to complement optical data in the mapping of rapid broad-scale flooding in Pakistan" *by* D. O'Grady et al.

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1. We would like to thank the reviewer for his detailed consideration of our submission and for the very constructive comments and advice, which we feel will add to and improve upon our proposed paper in a significant way. We address the comments in the order in which they were given:

2. While the presentation of the Pakistan case study is well done and certainly worth being published given the magnitude of the event that is certainly of interest for a broad community, I am far less convinced by

the development and testing of the new flood mapping technique. The authors state to have developed a new ASAR GM mapping technique (p.5784 l. 19). However, the method is only presented rather shortly (p.5777 l.10–19)

To clarify, our submission was not intended to present a new algorithm, and, as you say, we do use the region-growing function provided in the GRASS GIS package for the classification stage. We do agree that our use of the word developed in the sentence "A major limitation of the ASAR GM mapping technique developed here is...", to which the referee refers, is therefore not appropriate, and we propose that this changed simply to "used".

3. Also there is no comparison with other state-of-the-art methods (see, e.g., a recent paper from our group introducing a similar method based on thresholding,region growing and change detection; Matgen et al., 2011). Therefore it is difficult for the readership to evaluate the advantages and limitations of this method with respect to other methods.

It is true that such detection methods as authored by the reviewer could have been drawn upon, and we therefore propose to make reference to this fact in the revised submission.

4. I have the feeling that the main interest for the community could be the study of the added value of pre-flood baseline backscatter values for flood detection. For this, it would be interesting to carry out the same flood extraction method, but to replace the change detection images with the calibrated flood images in the region growing process. Is there any significant gain in performance to be obtained by using the reference image? Or would you get similar or better results with the “flood” image?

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This is indeed a valid point. We do feel that the difference between what may be achieved by a single calibrated image and that gained from the difference method is demonstrated in Figure 8 of our original submission, where two very dry regions (one south of Sibi, Area A, and the other, Area C, being a part of the Thar Desert), share similar backscatter values to the flooded valley. These areas, covering hundreds of square kilometres, are eliminated in the differencing process. We felt that a kappa analysis to further substantiate such broad-stroke effects would be trivial. We are happy, of course, to carry out such analysis if it is felt that it would contribute value to our submission. We will seek advice from the Editor on this matter.

5. In my opinion, another debatable point is the definition of the 2 dB tolerance criterion of the region growing. It is not clear from the manuscript how this parameter was fixed and how it influences the results of the case study. Is it obtained through an optimization with respect to the MODIS flood extent? The choice of this parameter, which arguably has a very significant impact on the results of this study, needs to be better justified. Ideally a sensitivity analysis should be added to the paper.

We take on board your comment and agree that this does need to be better explained. The choice of threshold was made by comparison with the MODIS signal, as visualized in Figure 9 of our submission. A sensitivity analysis would certainly be appropriate here, as you suggest, and we therefore propose to include one in the revised submission.

6. Introduction: While I like the introduction to this study, I think that more attention should be given to high resolution SAR imagery. In fact, I believe that the combination of coarse resolution and high resolution SARs provides significant advantages (arguably even more so than the combination of coarse resolution microwave and optical imagery), but

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this is hardly mentioned here. Also I feel that many relevant papers on SAR-based flood mapping are missing (see, e.g., Schumann et al., 2009 for an extensive review of existing techniques, and Horritt et al., 1999, Mason et al., 2007, 2010; Martinis et al., 2009; Matgen et al., 2011 for some existing techniques that come to my mind)

We were hoping here to concentrate on the benefits of the GM Mode data, with its excellent temporal frequency, low cost and, most importantly for this study, its availability for systematic acquisition within hours of the flood event commencing. Your SAR-based references are certainly worthy of making, and we therefore propose to do so in the revised submission.

7. p.5770, l. 24: it would be better to give the value of specific discharge (this value does not say much if the catchment size is not provided)

We see your point - This figure in isolation does not adequately portray the scale of the flooding at Munda at the time. We propose to amend the wording to cite this figure in the context of the irrigation headworks on the Swat River at Munda, which were built to a discharge capacity of 4.5 Mls^{-1} and which were damaged by the peak discharge of 8.5 Mls^{-1}.

8. p.5771, l.24: it is not only the operation mode that helps increasing the repeat coverage. You could also mention multi-satellite constellations such as COSMO SkyMed or the combination of SAR data from different missions.

Noted. These systems will be included.

9. p.5773 l.14 Please explain why you created this buffer.

The object of the procedure was to gauge the proportion of the Indus river with cloud-free exposure to MODIS Aqua and Terra through time, in comparison to the

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coverage available with ASAR Global Monitoring Mode (GM) data, in the context of flood mapping. A buffer must surely be created so that an area may be studied. Were this not the case, the alternative would be the analysis of coverage of the linear feature which would, in the case of the GM data, be less than a pixel in width for much of its course. The choice of size of the buffer was somewhat arbitrary, but given the scale of the overall study and the size of the flood, we feel that 50km is reasonable. In the region between Jacobabad and Dadu, the width of the flood approached this figure, for example.

10. p. 5776 l.14 How did you make sure that it was a "dry" image? Does this choice have a significant impact on the results or would any image acquired from the same orbital track provide similar results?

The answer to your question is fairly straightforward, but it is a good question and leads us to realize that some further clarification here would benefit our revised submission. The baseline image needs to be acquired at a time when the river is not flooded. However, the various environmental conditions prevalent at the time of the baseline image in conjunction with those at the time of the flooding obviously combine to produce various output values which can introduce some ambiguity. This will be addressed in more detail in the resubmission.

11. p. 5776 l.17 Table 2 is mentioned before Table 1.

This was not the case with the manuscript sent to Copernicus, and is a Laxex typesetting issue. We will address the matter if it recurs in the revised submission. Thank you, we should have spotted it.

12. p. 5777 23 Please add the Kappa statistics equation somewhere.

This will be done.

13. p. 5777 l.10 Is it really necessary that the flood pixels are adjacent to the main river channel? During the receding limb of the hydrograph I

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assume that there could be a disconnection. Also there could be some flooding because of the outcropping of the groundwater table and/or the accumulation of rainfall in depressions. I think that these potential limitations of the method should be indicated somewhere.

Yes, this certainly is a limitation which should be indicated, and this addition will be made in the revision. Certainly more sophisticated approaches could be adopted in an attempt to capture such isolated bodies of water, but we feel that this is outside of the scope of this submission.

14. p. 5778 l.23 please briefly introduce the MODIS-based flood classification of Brakenridge (2011).

This will be addressed.

15. p.5779 l.24 you refer to Fig. 8 before referring to Fig. 7

See item 11 above.

16. p. 5780 If I understood correctly all permanent and semi-permanent water bodies are removed from the SAR-based flood extent, while the MODIS data set includes all water bodies. Does this not impact the results of the analysis? Is there no way to exclude these areas from the analysis to make it more coherent?

This is true. However, where water bodies are permanent and semi-permanent, their extents are easily mapped and overlain if required. We are really only interested in mapping inundation outside of the current river coarse. The filling-in of permanent water after the flood classification and prior to testing would no doubt have increased the kappa statistic. However, we chose to leave the results as they were, with such limitations remaining exposed. Part of the reason for this is that the flood dynamics of the Pakistani rivers are complex, punctuated by sudden effects such as the breaching of the many levees which regulate their flow,

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and we preferred to make no assumptions as to any possible deviations to the normal channel flow. We will advise to this effect in the resubmission.

17. p.5782 l.6 I would replace “rate of advance of the flooding” by “propagation speed of flood waves”. p.5782 l.27 delete 1 x “to”.

These will be done.

18. Section 5.1 appears a bit disconnected from the rest of the paper. The paragraph over-simplifies the issue of integrating remote sensing data with hydrologic-hydraulic modelling tools. I can’t see any added value of this paragraph unless the issue is addressed in a much more exhaustive way. Moreover, there seems to be some confusion between hydrological models and hydraulic models (the latter being used for simulating the propagation of flood waves and flood extent).

The paragraph talks in general terms of the importance that satellite data now has in flood forecasting and mapping. The adjective ’hydrological’ describes the field of study in general terms, and we feel there is nothing wrong with assigning this to the numerical models used within this field, and this is therefore done correctly and without the confusion that has been suggested. The distinction between the use of satellite data and the prediction of flood dynamics based on models is more significant in the region studied, due to the levees which are widespread throughout the region, many of which are built ’privately’ and therefore remain unmapped [1]. We take your point that the paragraph may seem disconnected, and will remove it if the Editor agrees.

19. p.5786 l.2 If I remember correctly the planned launching date of SWOT is much later (please check).

[1] see http://tribune.com.pk/story/219602/private-dykes-on-public-land-may-lead-to-another-bout-of-floods/, for example

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Thank you for pointing out this mistake. The SWOT mission is indeed planned for launch in 2020.

20. p.5786 To be more exhaustive it would be necessary to also mention the upcoming ESA Sentinel-1 mission here.

    This will be done.

21. Thank you once again for your comments and advice. We are soon due to submit a revised manuscript, which will accommodate these and other comments as outlined in our responses.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 5769, 2011.