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Discussions

Interactive comment on "Application of CryoSat-2 altimetry data for river analysis and modelling" *by* R. Schneider et al.

R. Schneider et al.

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Reply to the review of Anonymous Referee #2

This paper describes the application of remotely sensed altimetry data from the CryoSat-2 satellite to large scale hydraulic modelling, using the Brahmaputra Basin as an example. While the paper is generally well written and clear, there are a few issues related to the focus and balance of the paper that will need addressing.

The remote sensing aspects of the study seem very well described, but the description of the hydraulic modelling is relatively weak. In this respect, the novelty of the work lies in the use of the Cryosat-2 data rather than the hydraulic modelling. In fact given the current research in large scale hydraulic modelling the approach used in the paper is overly simple. Moving beyond the "virtual gauge" is of great research interest and

I think this study has real value here, particularly with the fusion of drifting orbit and Envisat virtual stations. The filtering using a dynamic Landsat water mask is also of value and overall I think there is sufficient novelty in the work for publication.

While there are some issues to address, I do not think further modelling is required. I think most of the issues can be addressed with changes to the core text. There should be better reference to existing large scale hydraulic river modelling and more discussion/openness about the modelling limitations.

Reply: We thank the reviewer for constructive and insightful comments and suggestions. We fully agree with the reviewerthat the contribution of this paper is the integration of CryoSat-2 data into a hydrodynamic model (and not for the hydrologic-hydrodynamic modelling as such).

Some more specific points that should be addressed: (1) The work seems to miss some aspects of recent research that I would assume would be relevant to the work. For example no mention is made of studies that use ICESAT – another dataset that has been used for similar hydraulic model calibration. There is also no reference to the relevant work on channel representation in large scale 1d-2d modelling such as that of Neal et al (2015) (and previous studies).

Reply: We agree that the referencing may be too narrow in places and will include the suggested context in the revision.

Plan for revision: Include and discuss further references, as pointed out by reviewer.

(2) Why only use a 1d model when there are plenty examples of this scale of hydraulic model using 1d&2d? Essentially all the floodplain and braided river section details are being lumped into the single triangular cross-section, so I am not sure how valid the representation of the river/floodplain is in the end. It might work as a simple water level response function that can be calibrated (as demonstrated in the paper), but it losses any physically based reality in representing the river and its floodplain, thereby limiting

the value to the model for basin/river/floodplain studies. It is possible of course that the hydraulic conditions are such that the detailed representation of the channel is less important, such as found by Trigg et al 2009 on the Amazon. However there is no detail provided to show this is the case, for example what are the Froude numbers for the flow? It has not been demonstrated that the resulting model has value outside of the modelled scenario. I don't think that the model necessarily has to be redone, but I do think its limitations need more discussion.

Reply: Focus here is on accurate prediction of water levels and discharge, this is not a flood model. The main reason for choosing a 1D model was computational efficiency. It is correct that 1d-2d modelling at this scale is feasible, but probabilistic approaches using large ensembles of model runs would pose significant computational challenges. For example, the cross section calibration presented in the article using a genetic algorithm to find optimal parameters requires many runs (in the range of 10 000) of the model. Moreover, running a meaningful 1d-2d model would require accurate topography/bathymetry, which is unavailable for this braided and highly dynamic river system. The result of the cross section calibration, especially of step 2 where the amplitudes are being fitted, is consistent with the results by Trigg et al. (2009): with the chosen – simplistic – cross section representation we are able to reproduce observed water level dynamics. The authors however assume that the study is transferable to other rivers as well, given the availability of sufficient altimetry data. Even if a triangular cross section with varying angle will not be able to reproduce observed water level dynamics for all rivers, the same calibration procedure could be applied to other descriptions of cross section geometry. For example the same procedure should also work for the power-function cross section shape described by Neal et al. (2015): Instead of using a triangular cross section with the angle as only parameter, one could use the power function cross section shape and use i) only shape parameter s or also ii) both shape parameter s and bankfull depth h_full as calibration parameters. (assuming that the third parameter to describe the cross section in Neal et al.'s approach, bankfull width w_full, can easily be estimated from remote sensing data)

Plan for revision: Include some discussion as initiated by the reviewer and include some details on the hydraulic model and results as well as limitations

(3) More discussion is required on the uncertainty in flow produced by the rainfall runoff modelling and how it affects the hydraulic modelling.

Plan for revision: Include some details and discussion of the uncertainty of the rainfall-runoff models. Potentially including data from the calibration catchments.

(4) There is reference to the dynamic nature of river channel with regards to the water mask, but no discussion of the how important this geomorphology might be to the simple triangle river channel model used.

Reply: For this simple 1D model, with its synthetic cross sections, we assume that the change of the river channels does not significantly affect the water level-discharge relationships. Also, Mirza (2003) found rating curves at the Brahmaputra to exhibit fairly constant Q-h relationships over decades, leading to the conclusion that the Brahmaputra River is in "dynamic equilibrium".

Plan for revision: Include some discussion of this

Reference: Mirza, M. M. Q.: The Choice of Stage-Discharge Relationship for the Ganges and Brahmaputra Rivers in Bangladesh, Nord. Hydrol., 34(4), 321–342, doi:10.2166/nh.2003.019, 2003.

(5) I am not clear on how the SRTM is actually translated into the triangle river channel. Has the raw SRTM data been processed to remove the vegetation bias? What is actually used for the 1d triangle, the width and depth of the river extracted from the SRTM? If so maybe river width from landsat would be better for the width and estimate of depth from geomorphological relationships (Leopold, and Maddock, 1953) would be better? What size are these calibrated triangles. Do they bear any resemblance to the real river sections?

Reply: What is referred to as "reference" cross sections in the paper was visually extracted from satellite imagery and the SRTM DEM in a consulting project preparing the Ganges-Brahmaputra hydrologic model used in this paper. The real river cross sections (of this multi-channelled river) will of course be very different from these simplistic cross sections. However, this is not so important in a 1D model, as long as the relationship A = A(h) and P = P(h) are realistic, i.e. we need to get the relationship between flow cross sectional area and wetted parameter right.

Plan for revision: With this context, maybe the term "reference" cross sections is a bit bold; and this limitation should be discussed when showing the "reference" water levels in Figure 7.

(6) Manning's is mentioned but no values given. Given its direct control on water levels and it should have some link to expected values it should not be omitted. Given the crude nature of the cross-sections and the fact that Manning's will compensate for lots of missing processes in this regard, I am not sure the calibrated Manning's values will bear resemblance to what might be expected for such a river.

Reply: Manning's number only has been calibrated against discharge (timing of discharge) at the outlet station Bahadurabad. Its value is 0.029, which is slightly low but can be considered plausible – see for example http://www.fsl.orst.edu/geowater/FX3/help/8_Hydraulic_Reference/Mannings_n_Tables.htm.

Plan for revision: Include a short description of the Manning calibration; and also extend Figure 3 of the article to make the different calibration steps more clear (this is also something pointed out by another reviewer)

Refs: Neal et al, 2015. Efficient incorporation of channel cross-section geometry uncertainty into regional and global scale flood inundation models, Journal of Hydrology. Trigg et al 2009. Amazon flood wave hydraulics. Journal of Hydrology. Leopold, and Maddock, 1953, The hydraulic geometry of stream channels and some physiographic implications, U.S. Geol. Surv. Prof. Pap.

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