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

Anonymous Referee #1

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GENERAL COMMENTS:

The paper is interesting because it shows a practical use of Cryosat-2 data for a hydrodynamic modelling. So far, a few studies are available on this issue in the scientific literature. Therefore, I found the paper highly timely and appealing.

The manuscript is well written and easy to follow, even if some aspects should be better clarified. The main issues concern: 1) the specification of the paper purpose, 2) the description of the hydrodynamic model and 3) the procedure of optimization of the cross-section geometry. Moreover, I have doubts concerning the study area characteristics. The evaluation of the Cryosat-2 data performances cannot be exhaustively tackled if no data are available for the validation.

SPECIFIC COMMENTS:

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Introduction:

1) The purpose of the study is not well specified. I suggest the authors to add in the introduction a couple of sentences on this aspect also to introduce the model and the datasets used: why do they use 1D model for this complex river? Why software MIKE 11? Why Cryosat-2 and Envisat?

2) I believe that the background should be addressed following the purpose of the paper. The literature review described in the introduction is quite extensive, but it should be more focused on the use of radar altimetry for the calibration of the hydrodynamic models or the cross-sections geometry, mentioning similar studies (see references). For example:

Domeneghetti et al. (2014; 2015) compared the performances and analyzed the uncertainty of ERS-2 and ENVISAT radar altimetry in the calibration of the manning coefficient of the Hec-RAS model along a river reach of the Po river in Italy.

Yan et al. (2014) calibrated the manning roughness coefficient and the depth of the cross sections for the LISFLOOD-FP model in the Danube River with the use of water surface level derived by Envisat radar altimetry.

Biancamaria et al. (2009) compared the water levels derived by 22 TOPEX/POSEIDON VSs with the ones simulated by large scales coupled hydrological-hydraulic model of the Ob river in Siberia calibrating the river depth and Manning' roughness coefficient.

3) I suggest citing Tourian et al. (2016) for the merging of satellite altimetry. They analyzed different time series from Envisat, Saral/Altika, Topex/Poseidon and Cryosat-2 in the Po, Congo, Mississippi and Danube rivers.

Study area:

1) Why do the authors focus on Brahmaputra River? Cryosat-2 data are available for rivers where the in-situ data could be easily obtained. The risk to use a poorly gauged river (or as in this case a river where the data are not publicly available) is to be not

able to validate the procedure in a proper manner.

2) I have doubts on the use of “calibration” term in the text: “discharge calibration” or “water level calibration”. The calibration is referred to the parameters of the model in order to reproduce the measured discharge or water level. I guess that, in this case, the authors calibrate the parameters of the hydrodynamic model and, then, compare the simulated discharge with the observed one. Therefore, I suggest to pay attention.

Data and Methods:

1) This section is quite unbalanced. The description of the satellite data, especially for the water mask, is too long with respect to the hydrodynamic model.

2) From Fig.2 the model river line seems very different from the natural water course. The authors should clearly describe how it was derived.

3) About the hydrodynamic model, more details and clarifications are necessary.

3.a) First, the authors state that Bahadurabad is along the Brahmaputra river, but in Figure 1 it seems outside the contour of the basin. If we suppose that the gauged site is available inside the basin near the outlet (and hence, the contour is wrong), it could be sufficient for calibrating the rainfall-runoff model. Why do the authors extent the rainfall-runoff model to the Gange Basin? Moreover, how do they transfer the parameters for the 11 subcatchments to the remaining ones? Please specify.

3.b) About the hydrodynamic model, the procedure of calibration of the cross section geometry is not clear. If Cryosat-2 and Envisat do not refer to the same cross-section (VS), it should be specified how step 1 and step 2 should be applied. Indeed, some details are given in Table 1, but I believe that a deeper description should be added in the text.

Moreover, after the second calibration step, in Fig.3 the flow chart indicates that the procedure is iterative. I do not understand at what level the iteration happens. I think that in order to obtain a calibration the objective function should be unique and minimize

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the RMSE for both the steps in parallel. I think this is a very important part of the procedure, therefore I suggest to add details and clarifications. Indeed, page 10 Lines 28-30 should be moved in this section.

3.c) In the hydraulic model, no mention is given to the roughness manning coefficient. Even if it was not specified in the text, I think the authors used a unique coefficient value for the entire river. Please add some details.

3.d) How do you set the initial condition of the model? What about the boundary condition at the downstream site? Please specify.

4) Which is the length of the river simulated with the hydraulic model?

Results:

1) Why do you choose 20 m for defining the outliers of the Cryosat-2 values?

2) The authors state that the manning's number is calibrated. Which is the value? Is it plausible for this river?

3) In the text, it is mentioned that the investigated river reach is the Assam Valley. Figure 7 shows the water levels for a river reach from ∼1950 km to 2800 km. Figure 8 shows the VS at 2839.019. Could the authors add the length of the analyzed river (not well specified) and update Figure 7 for the actual length?

Conclusions:

1) The authors state that "SRTM products do not provide sufficient information to create a hydrodynamic model reproducing accurate water levels or inundations areas". I believe the river is not enough gauged to evaluate the performance of SRTM. In a different study area, the authors could evaluate the accuracy of SRTM in comparison with the proposed procedure, but in this case the only conclusion that can be drawn is that SRTM and radar altimetry gave different results.

2) Could the procedure be transferable to other case studies? Could the authors sug-

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gest the minimum width to apply it?

TECHNICAL CORRECTIONS:

Please, remove capital letter after the colon.

Page 3, Line 19: “Mike 11 software”: a previous citation of the hydraulic model MIKE 11 used for the analysis is necessary. Please specify if it is a hydrological or hydraulic model and add some references.

Table 1: why 27 cross sections? The Envisat tracks are 13 as reported in the pages 8 Line 15.

References

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