Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-243-AC1, 2016

Hydrology and Earth System Sciences
Discussions
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Interactive comment

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

R. Schneider et al.

rasch@env.dtu.dk

Received and published: 19 September 2016

Response to the review of Anonymous Referee #1.

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.

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Reply: We thank the reviewer for constructive feedback on this article. Below, we explain in detail how the individual issues will be solved in the revision.

SPECIFIC COMMENTS:

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?

Reply: The choice of CryoSat-2 is due to its unique drifting orbit which provides water level profiles with high spatial resolution. In combination with (any, not necessarily Envisat) repeat orbit altimetry data providing water level time series at virtual stations these data can be used to calibrate the water level dynamics (which hopefully also becomes more clear in section 3.4). So, the purpose of the paper is to find out what CryoSat-2 can do for river modelling. The 1D model was used because, for the study area, we lack (access to) precise DEMs or bathymetry data. Hence, a 1D model with synthetic cross sections was used – the focus of the study is to simulate water levels (and discharge) in the river, however not flood extent. Furthermore, at the large scale of the model (1000 km plus of river) anything else than a 1D model will become computationally heavy in calibration (and later data assimilation experiments) Plan for revision: Extend the first paragraph of section 1 to include more details on the purpose.

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).

Reply: Thanks for the list of interesting and relevant references. Plan for revision: We will incorporate those into the literature review part.

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.

Reply: Yes, that is correct, the data over the Brahmaputra River will be hard to validate. The alternative would have been to use another, better gauged river. The choice of suitable rivers however is not very large, because it needs to be of sufficient width, preferably flowing in west-east direction and (fairly) unregulated. One common example is the Amazon River. In this case however the river is being monitored on the ground, hence the information gained from satellite altimetry is less crucial. In this tradeoff it was decided to go for a study region where in-situ data actually is lacking, making application of remote sensing data crucial. Also, for the Amazon River, there are also plenty of altimetry studies available.

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.

Reply: The authors are not sure they entirely understand the comment. Maybe the reviewer could try to clarify?

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.

Reply: The authors consider the review of the different filtering methods/water masks used by the inland altimetry databases (paragraph 2 of section 3.1.1) interesting and relevant. Does the reviewer disagree? If so, why? Paragraphs 3 and 4 of section 3.1.1 however will be shortened. Plan for revision: Shorten paragraph 3 and 4 of section 3.1.1 (and expand the hydrodynamic model description, also see below)

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.

Reply: This disagreement between river mask (∼natural water course) and 1D model river line comes from the fact that the river line is derived from the SRTM DEM by hydrologic routing performed in ArcGIS. Such a course will deviate from the natural water course for Brahmaputra River in the Assam valley mainly because of i) inaccuracies in the SRTM and the relatively flat river valley and ii) changes in the river's course over the years since the acquisition of the SRTM data in 2000. (The hydrodynamic model however will be insensitive to changes in the river's course, as long as the total length of the stretch remains approximately the same) Plan for revision: State the disagreement (and reasons for it) more clearly in section 3.3 where it is mentioned that the 1D model river line is derived from the SRTM DEM.

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

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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 rain-fallrunoff model to the Gange Basin? Moreover, how do they transfer the parameters for the 11 subcatchments to the remaining ones? Please specify.

Reply: Bahadurabad station is assumed to be placed at the outlet of the model. It is correct that the outline of the model displayed in Figure 1, which is the basis for the rainfall-runoff model subcatchments, does not agree when the location of the river line is considered in this part of the model. The reason for this is inaccuracies in the SRTM DEM that was used for the subcatchment delineation. Near Bahadurabad station, where the river valley is very flat this lead to the subcatchments ending approximately 10 to 20 km west of where they actually would be expected to end – when one looks at the actual location of the river. However, in the model (remember also that the NAM rainfall-runoff model is a lumped model), the subcatchments are correctly attributed to/draining into the river of the hydrodynamic model. The Brahmaputra basin model used here is, as mentioned in the beginning of section 3.3.1, part of a larger model covering both the Ganges and Brahmaputra basins. This model originates from the Danish hydrologic Institute (DHI). However, no part of the Ganges basin model is used for the work described in the article. So, the model was not really extended to the Ganges basin as the reviewer understood, but it was more that the available information from the Ganges basin (i.e. the rainfall-runoff model parameters) were used for the Brahmaputra basin. Parameters were transferred between subcatchments using simple heuristic rules. Because of the unfortunate situation that only 11 (out of 86 in total) subcatchments could be calibrated against in-situ discharge at their outlets, the other subcatchments had to be given parameters that were derived from those 11. Parameters in the NAM model have some physical meaning, so differences in topography for example can guide in how to transfer parameters from one catchment to another. Furthermore, total runoff from all the aggregated Brahmaputra catchments could be checked against the discharge at Bahadurabad station – the total water balance bias between simulated and observed discharge at that station is only 2%, as mentioned at the end of section 4.2. Plan for review: Try to make the difference between the Ganges- and Brahmaputra basin more clear and add a few explaining words on the transfer of NAM model parameters.

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.

Reply: Yes, the different number of cross sections is confusing. The data from Envisat does not cover exactly the same river section as the CryoSat-2 data used, hence not all the same cross sections are calibrated. With Envisat, cross sections from river km 2050 to 3050 could be calibrated, whilst with CryoSat-2 data cross section between rive km 1950 and 2800. Hence, only the overlap from river km 2050 to 2800 can be considered fully calibrated. Plan for revision: state the sections of the river with overlapping satellite data from Envisat and CryoSat-2 clearly (also rework Table 1). Furthermore, Figure 3 (the flowchart of the whole calibration process) will be reworked for the revision, hopefully adding more clarity.

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 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.

Reply: Yes, the authors agree that Figure 3 can be improved. We however are not sure how to go about minimizing the RMSE for both steps in parallel, as suggested by the reviewer. The two different objectives from step 1 and step 2 could be merged into one optimization. This however would also increase the complexity of the problem, as both sets of decision variables would have to be considered. This probably will increase the computational demand of the already demanding optimization problem. Hence, because the especially the sensitivity of the water level amplitudes to small changes in the cross section datums is very low, this iterative approach is considered sufficient. Given the low sensitivity of, in other words, the objective of step 2 to changes resulting from step 1, the iteration usually can be ended after a run of step 1. Plan for revision: Explain the iterative calibration process in more detail as outlined in the reply above, and improve Figure 3 for more clarity.

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.

Reply: The Manning coefficient was calibrated to one unique value along the entire river. Plan for revision: Include a few lines on the manning coefficient calibration and the resulting value.

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

Reply: The initial conditions of the hydrodynamic model are taken from a hotstart of the model. More important however is the hotstart of the hydrologic part of the mode, i.e. the NAM rainfall-runoff models, because those models have states with much “longer memory”. The hotstart of the NAM rainfall-runoff models was created by running the initial calibration period of the model, 2002 to 2007, 30 times, reusing the final state of the respective prior run as a hotstart. After 30 iterations, or 180 years, all states with long memory (mainly groundwater and snow storage) have reached steady-state. This steady state then is used as the hotstart. A time series of water levels at the downstream boundary of the model (which lies ∼180 km downstream of Bahadurabad station) could be obtained for the years 2001 to 2009 – outside that range a climatology of these values was obtained. In any case, discharge and water level at Bahadurabad station (so the downstream end of the area of interest) is insensitive to this downstream boundary condition. Plan for revision: Include these details in the text.

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

Plan for revision: Include details in the text.

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

Reply: This is a value that was chosen after inspecting the differences between SRTM and CryoSat-2 elevations along the river. In general, CryoSat-2 observations group nicely around the SRTM values, and only very few clear outliers do exist – these will be removed with the chosen threshold of 20 metres deviation.

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

Reply: The resulting value is a Manning's n value of 0.029, which is slightly low for the given river, but should be considered plausible (compare for example http://www.fsl.orst.edu/geowater/FX3/help/8_Hydraulic_Reference/Mannings_n_Tables.htm) Plan for revision: (also see above) Include a few words on the manning coefficient calibration.

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?

Plan for revision: This will be updated in accordance to what is written as reply to comment 3.b) above.

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

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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.

Reply: The authors agree that this statement may be too simplistic. However, the authors still believe that the SRTM – at least as a raw product – is not precise enough to directly derive a hydrodynamic model accurately reproducing water levels. This can be shown for example by the significant improvements that Jarihani et al. (2015) could achieve when deriving cross sections from SRTM DEM, and then subsequently correcting the SRTM DEM for vegetation and other issues (Table 4 in their article). Their baseline for the comparison is derived from ICESat data, which they could validate against in-situ data to have a RMSD of only 0.23m. But even when the SRTM DEM was vegetation smoothed and hydrologically corrected, its RMSD compared to a cross section from ICESat was above 1.1m. Another example is the work by Md Ali et al. (2015) using a DEM from lidar data with 1m resolution to set up a 1D hydrodynamic model and comparing it to, amongst others, the same hydrodynamic model based on the SRTM DEM. They found the resulting simulated water levels of the SRTM DEM based hydrodynamic model to have a MAD of 0.76m compared to the same levels from the lidar based model. With the proposed procedure, the water levels will be fitted to CryoSat-2 observations. Based on the literature cited the authors assume that fitting the simulated water levels to CryoSat-2 data also means a better fit to real water levels than what can be achieved by setting up the hydrodynamic model based on the SRTM DEM only. Remember also the difficulties of obtaining an estimate of bathymetry from DEMs, whilst the suggested procedure does not require any knowledge of bathymetry.
References: Jarihani, A. A., Callow, J. N., McVicar, T. R., Van Niel, T. G. and Larsen, J. R.: Satellite-derived Digital Elevation Model (DEM) selection, preparation and correction for hydrodynamic modelling in large, low-gradient and data-sparse catchments, J. Hydrol., 524, 489–506, doi:10.1016/j.jhydrol.2015.02.049, 2015. Md Ali, A., Solomatine, D. P. and Di Baldassarre, G.: Assessing the impact of different sources of topographic data on 1-D hydraulic modelling of floods, Hydrol. Earth Syst. Sci., 19,

631–643, doi:10.5194/hess-19-631-2015, 2015. Plan for revision: Reformulate that statement, stating the assumptions made, including the references mentioned.

2) Could the procedure be transferable to other case studies? Could the authors suggest the minimum width to apply it?

Reply: Yes, the authors expect that this procedure can be transferred to other case studies.. A minimum river width however seems to be hard to define, as the ability of satellite altimeters to reliably measure water level in (narrow) rivers depends (besides the actual instrument and processing) not only on the river width, but also on the topography of the river valley – see for example what is discussed in connection with the results shown in Figure 5 and the article by Dehecq et al., 2013. Plan for revision: Add a few words on the transferability of the developed procedure.

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.

Plan for revision: Yes, this will be included in the revision.

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

Reply: Yes, there exist only 13 virtual stations along the Assam valley. Those virtual stations however can be used to calibrate both neighbouring cross sections (as the virtual station usually is not placed directly on a cross section). Furthermore, angles for cross sections lacking neighbouring virtual stations were linearly interpolated between the next cross sections. Plan for revision: Clarify these things together with what is mentioned in the reply to comment 3.b)

References 1) Domeneghetti A., Tarpanelli A., Brocca L., Barbetta S., Moramarco T., Castellarin A., Brath A. (2014) The use of remote sensing-derived water surface data for hydraulic model calibration. Remote Sensing of Environment, 149, 130-141. http://dx.doi.org/10.1016/j.rse.2014.04.007 2) Domeneghetti A., Castellarin A.,Tarpanelli A., Moramarco T. (2015) Investigating the uncertainty of satellite altimetry products for hydrodynamic modelling. Hydrological Processes, 29(23), 4908-4918. http://dx.doi.org/10.1002/hyp.10507 3) Siddique-E-Akbor, A. H., Hossain, F., Lee, H., & Shum, C. K. (2011). Intercomparison study of water level estimates derived from hydrodynamic–hydrologic model and satellite altimetry for a complex deltaic environment. Remote Sensing of Environment, 115, 1522–1531. 4) Tourian M.J., Tarpanelli A., Elmi O., Qin T., Brocca L., Moramarco T., Sneeuw N. (2016) Spatiotemporal densification of river water level time series by multimission satellite altimetry. Water Resources Research, 52. http://dx.doi.org/10.1002/2015WR017654 5) Wilson, M.D., Bates, P. D., Alsdorf, D., Forsberg, B., Horritt, M., Melack, J., et al. (2007). Modeling large-scale inundation of Amazonian seasonally ïnËĞC′ ooded wetlands. Geophysical Research Letters, 34,L15404. http://dx.doi.org/10.1029/2007GL030156. 6) Yan K., Tarpanelli A., Balint G., Moramarco T., Di Baldassarre G. (2014) Exploring the potential of radar altimetry and SRTM Topography to Support Flood Propagation Modeling: the Danube Case Study. Journal of Hydrologic Engineering 20(2). http://dx.doi.org/10.1061/(ASCE)HE.1943-5584.0001018

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