Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2017-217-RC2, 2017

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Water levels of the Mekong River Basin based on CryoSat-2 SAR data classification" *by* Eva Boergens et al.

Anonymous Referee #2

Received and published: 24 June 2017

The authors use Cryosat-2 SAR data for inferring water levels over the Mekong river system. As with the earlier pulse-limited radars, applying SAR for inland water bodies requires a (static or time-variable) land-water mask for separating water echoes from land returns, that is usually derived from optical and/or radar remote sensing. In this article, the authors suggest to derive the mask from the SAR Delay-Doppler stacks itself, using a standard classification method. It must be noted that Cryosat-2 levels, due to the satellite's unusual 'repeat' orbit, are difficult to validate.

SAR promises to enable water level measurements for rivers of width down to few 100 m or even less, and the availability of river masks poses a challenge, in particular when seasonal inundation is present. The work is timely and touches upon a relevant topic. However, the scientific hypothesis and the paper's objective are not well-described and

Printer-friendly version

Discussion paper

it is difficult to read. In large parts, the article is written in an explorative style: the authors apply a certain sequence of approaches and report about success, but it is not explained why these particular approaches are chosen nor are systematic tests provided. This applies to the classification approach (k-means), and the same goes for the features chosen to be used for classification. The authors jump back and forth with approach and validation. I would suggest a more systematic writing: data, method, results, validation, interpretation. Also, it would be helpful to have 1) a flowchart for the approach including region subdivision, classification, retracking, and outlier removal , and 2) a flowchart for the validations.

Other remarks

Page 2 lines 24-30: Here, the authors somehow suggest the range-integrated power, RIP, provides a kind of independent observation that is not available for other altimeters. Although it is true in a literal sense I find this slightly misleading: In fact, both the RIP and the SAR waveforms are derived from the stacks (Fig. 2) which contains the primary observable. In fact all the features they used can be interpreted as properties of the stack matrix. This leads to the question whether the authors actually use the multi-look stacks for focusing on the rivers when deriving water levels? This should be answered in section 3.

In my printout of figure 1, the upstream / mid-stream / downstream mask hachures are not shown as indicated in the inset.

The authors argue that for the smaller rivers of the Mekong basin no reliable land-water mask is available. But there are definitely regions of the world where very precise land-water masks are available (e.g. all EU) – why not testing the classification approach properly for such a region? If this paper is meant to provide a new method, it would be perfectly ok to add comparisons in a test region outside the Mekong.

Eq. (1): the terminology is awkward. Peakiness pwf is not a vector, why is the symbol bold? Max(wf) should read max-over-I wfi

Printer-friendly version

Discussion paper

Can the authors please provide an example for the RIP asymmetry parameter caused by a realistic river sloped, assuming the satellite flies along the river (from 246 single looks)?

From the subdivision of the overall basin in three regions, the authors find that in the overlap regions the classification (and thus water levels) does not always agree. That's worrysome but may be expected given the approach. But what is the implication for applications?

After classification, the water surface is identified in retracked levels through searching for a horizontal (or, I guess, sloped) line. Outliers at the margins should tell about the misclassification in the first step. Is it possible to quantify this, e.g. in % of identified water surface from k-means vs. the straight-line in water levels?

k-means can be seen as a special case of maximum likelihood classification under assuming Gaussian distribution and spherical clusters – the distance measure is not weighted in the original method. But weighted k-means may be appropriate whenever features have different geometric meaning and/or units. Are all features equally weighted, does this really makes sense?

7 Conclusions: 'We demonstrate in this study the possibilities of classifying CryoSat-2 SAR data in the Mekong river basin and using this classification for water level extraction'. This statement carries no information at all – please be concise and provide real conclusions (the above is not even a summary).

Eq. (3) While (3) and (4) may have been 'derived' from the OCOG retracker, they represent standard statistical measures. I find it misleading to refer to the OCOG in this respect, which simply makes use of the same statistical moments.

There are some thresholds chosen for the outlier selection based on the near-annual repeat of C-2, height difference of 7 m, 10km / 30day spacing in the second step. It is said these are based on a conservative approach, but more should be provided on

Printer-friendly version

Discussion paper

how robust the overall results are with respect to these thresholds.

Appendix: All four figures have the label A1?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2017-217, 2017.