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Hydrology and Earth System Sciences Discussions

Interactive comment on "Estimating flooded area and mean water level using active and passive microwaves: the example of Paraná River Delta floodplain" *by* M. Salvia et al.

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General assessment: The manuscript submitted by Salvia et al. describes a method combining active and passive microwave remote sensing imagery for the monitoring of mean water levels in vegetated river floodplains. ENVISAT ASAR images are used to estimate the fraction of the flooded area and together with brightness temperature observations from AMSRE and model simulations the approach provides estimates of water level in vegetated floodplains. I have to admit that I am not very familiar with passive microwave remote sensing technology and for this reason my review mainly

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Interactive Discussion

Discussion Paper

focuses on the active remote sensing aspects of this study and, more particularly, on the added-value of combining active and passive microwave remote sensing observations. The main appeal of the technique introduced here is that with AMSR-E coarse resolution remote sensing imagery very high temporal resolutions can be potentially achieved. However, the model used by the authors of this study makes use of the "fraction of flooded areas" derived from moderate resolution active remote sensing imagery (with associated lower temporal resolution). Unless very strong assumptions are made concerning the changes in flooded area between acquisitions, the method critically depends on the availability of ENVISAT ASAR images in Wide Swath Mode. This means that it is important to discuss the advantages and shortcomings of this method over water level estimation methods that make use only of active remote sensing imagery (see, e.g., Alsdorf et al., 2009, Matgen et al., 2007). In my opinion the main advantage of the method introduced here could be that it is rather straightforward (when compared to the direct measuring techniques based on interferometry) and that it does not require any topographic data as input data (when compared to the indirect measuring techniques based on the fusion of flood edges with DEMs). However, I have some doubts remaining concerning the accuracy and reliability of the method. In this respect it does not help that the paper uses rather poorly the available ground measurements for evaluating its findings and better supporting its conclusions.

- A critical comparison between this method and other methods proposed in the literature will be included, also considering the indications of the reviewer.

- It is important to mention that the method depends on ScanSAR data, not Envisat in particular. Any time series of the same ScanSAR data (Envisat, Palsar, Radarsat, Cosmo) will work. Moreover, all the other microwave flood monitoring techniques require images, and usually impose more constraints on data and ancillary information of the area (e.g. coherence or DEMs).

- The work proposed in this paper is mostly methodological. However, in the Conclusions we anticipate a further development, aimed at fully exploiting the revisit time of passive systems.

- In order to better evaluate the results, we have now considered in-situ measurements of water level collected in 4 stations. Results are shown in Figures 1 and 2 (available at the end of this document), confirming the trend of Rosario station.

In fact, the objective of the paper is “to estimate both the fraction of inundated area and the mean water level inside the wetland” (p.2899 l.4). With the data at hand it is impossible to assess if these objectives were reached. All we see is that the remote sensing-derived “mean water level” increases steadily over the investigation period. The fact that water levels recorded at a single river gauge increase in a similar way does not provide strong evidence for the validity of the approach. Moreover, the paper does not include an evaluation of the SAR-derived “fraction of inundated area”. Finally, I found some assumptions not defendable or at least debatable (e.g. “the fractional area of permanent water bodies is constant” or “the temperature difference of flooded land is constant”). However, no particular effort is undertaken to verify these hypotheses. In general, the results section is very short and there is hardly any discussion of the results. There are certainly elements of this study that are of interest for the community. The method introduced here is potentially useful for monitoring water levels over large areas in densely vegetated wetlands. However, the paper in its current form suffers from a quasi-inexistent critical evaluation of the results. Since the manuscript does not include a comparison with state-of-the-art solutions, it is difficult to understand the merits of the proposed method.

- Validation of flood area and water level at this scale is usually difficult. The only kind of validation performed is based on other kind of remote sensing data. For example, NDVI is also sensitive to flood condition, but not in heavy vegetated areas with large cover fraction. However, there is an indirect proof of the method related to the increase of flooded fraction when WL recorded at multiple river gauges reaches the emergency level in major parts of the floodplain (see Figures 1 and 2).

- In order to critically evaluate the method adopted to derive the fraction of inundated area, the work has been done by considering different thresholds. Several flooded fraction maps have been developed and the sensitivity to thresholding has been evaluated. In Figure 3 we show the temporal trends of flood fractions for different thresholds, while in Figure 4 we show the corresponding temporal trends of water level. These new figures will be included and discussed in the paper.

- A discussion about state-of-the-art solutions will be included in the introduction, also following the reviewer's suggestions.

- About the validity of the hypothesis adopted, a more detailed discussion will be included in the paper. We stress that in Section 2.3 we were describing the assumptions made by Sippel et al., 1994, rather than introducing our assumptions. The statement "the fractional area of permanent water bodies is constant" refers to "permanent" water bodies, such as lakes, and not to occasionally flooded areas. Therefore, we think that it is a correct assumption. On the contrary, we agree with the reviewer about the criticism of the other assumption ("the temperature difference of flooded land is constant"). In fact our work is just aimed at overcoming this rough simplification. In [Sippel94], the objective was to estimate the flooded fraction from the observed deltaT data. However, to consider deltaTflooded constant in time and the same for all the different kinds of vegetation inside the study area is debatable. Indeed, a specific section of [Sippel94] was entirely dedicated to the statistical properties of the deltaTflooded estimated from images, since a large dispersion in its values was observed. In this work, we do not assume that deltaTflooded is constant in time. Indeed, we are going to consider that it changes in time according to the water level inside the vegetation. Nevertheless, we will consider that it is constant for all the different kinds of vegetation inside the study area. Since all vegetation types in the area are marshes with a similar height, this assumption is justifiable. Now, it is logical to assume that deltaTflooded is a function of WL. When WL = 0, deltaTflooded = deltaTNonflooded, and for WL > h, where h is the mean vegetation height, deltaTflooded = deltaTwater. The exact behavior of deltaTflooded as a function of water level in the range $0 \leq WL \leq h$ is related to the geometrical and dielectrical properties of the marsh vegetation, and has been studied using the interaction model. Our work is just aimed at considering two variables (flood fraction and water level) instead of one.

Specific comments: p.2896 l.11 forecasting instead of forecast p.2896 l.14 please delete “strong”

- These changes will be included in the text.

p.2896l.15 I don’t necessarily agree with this statement, for some applications, e.g. flooding in built-up environments, spatial resolution matters more than temporal resolution. Hence, it is debatable whether it is better to have more images with lower resolution or fewer images with higher resolution. SAR high-resolution constellations (e.g. Cosmo SkyMed) might be seen as a promising way forward to have both.

- Our statement was written introducing a study in the South American environment, where large rivers produce occasionally flooding effects of huge extension in vegetated land. We agree that the trade off between spatial and temporal resolution depends on the application, and we will formulate the statement considering this comment.

p.2896 l.23-25 “adopt the absolute difference: : :” adopt for what? Detection of flooded areas? Not very clear, please clarify.

- The paper by Choudury et al., 1986, exploits the polarization difference for several applications. We will formulate the statement more clearly.

p. 2896 l. 24 define acronym PI p. 2896 l.25 delete “at all”

- These changes and clarification will be included in the text. PI means Polarization Index, and is the difference between vertical and horizontal brightness temperatures normalized to the average value.

p.2897 l.5 It is not clear what is meant by “moderate” and “high” water levels as even

HESSD

8, C2162–C2172, 2011

Interactive Comment

with “moderate” water levels the height of the emerged vegetation is reduced. Is the moisture of a flooded soil having an influence on the brightness temperature?

- We intended “in presence of a moderate amount of water”, that is at the beginning of an inundation or a rainfall. In this part of the Introduction we were discussing the general behavior of Polarization Index. It increases with soil moisture. However, in the site considered by us the soil below marshes is wet even in normal conditions. Therefore, the effects are related to the decrease of emerged vegetation. This topic will be clarified in more detail in the text.

p.2897 l.7 replace “Indexes” by “indices”.

- This correction will be included in the text.

p.2897 l.15 you could add the double bounce from buildings (or geometric structures in general, including vegetation)

- The work is focused on soil covered by vegetation. In any case, we will try to give a more general formulation.

p.2897 l.20 “at C-band, the overall effect produced by a moderate flooding is an increase of the backscattering coefficient due to an increase of the double bounce contribution”. It needs to be emphasized that this applies only to flooding under canopy. On bare soils and on most lightly vegetated soils the effect of flooding would be to decrease the backscattering coefficient due to specular reflection (i.e., efficient redirection of the wave incident upon a smooth surface away from the source). Please clarify.

- In the previous line we stated “Flooding reduces the surface contribution, due to the decrease of roughness”. In any case we will stress more clearly that we are considering vegetated areas.

p.2897 l.24 on most surfaces (e.g. bare soils) there is no relation at all between the backscattering coefficient and the water level. In the presence of a flooded soil only

HESSD

8, C2162–C2172, 2011

the roughness of the water surface impacts the backscattering. Please clarify.

- As for the previous comment, we will stress more clearly that we are considering vegetated areas.

p.2898 l.1 replace "ideal" by "good"

- The change will be included in the text.

Introduction: The introduction mostly focuses on microwave remote sensing of floods under a canopy. However, in many cases where flooding occurs on bare soils, grasslands or built-up environments, the most relevant flooding-related backscattering mechanisms are quite different. Hence it is necessary to highlight these differences. Also it is worth mentioning that other techniques exist to estimate water level from space. For example, the fusion of remote sensing-derived flood boundaries with topographic data provides distributed water elevation data from space. You may add references to Matgen et al., 2007; Schumann et al., 2007 and Zwenzner and Voigt, 2009. You may also mention direct measuring techniques (e.g. Alsdorf et al., 2007) to provide a more complete picture of available techniques.

- This discussion will be in the text. However, the mentioned techniques focus on a different kind of water level retrieval applications, more concerned with high spatial resolutions, low spatial extent and low temporal resolution. The Introduction is focused on the conditions and the requirements of our study area.

p.2901 l.17 define acronym IFOV p.2902 l.9 proportional to the sum

- The corrections will be included in the text.

Fig.3 Why not show all the ASAR WSM images here? Can you show the boxplots or histograms of backscattering coefficients as this would show the increase of the spread in backscattering values? In my opinion the increase of backscatter due to "moderate flooding" also comes from wetter soils in the non-flooded parts of the AOI and not only from the double bounce effect of the flooding. This is important, as there is a risk of

HESSD

8, C2162–C2172, 2011

Interactive Comment

overestimating the fraction of inundated areas if the backscattering increases because of soil moisture rather than because of a double bounce effect under a canopy.

- It is important to note that, since this is a wetland area, soil is normally covered with marsh vegetation (there is no bare soils) and normally wet. Therefore, changes in sigma-0 should come from flooding. In order to test this, we computed the flood fraction using an asymmetric threshold (i.e. 2 dB of increase OR 1dB or decrease), but the result is very close to the case of a symmetric threshold with 1,5 dB of Increase OR 1.5 dB of decrease. See Figure 3 at the end of this document. This agrees with our assumption, since soil moisture can produce only an increase of sigma-0, while the reduction of emerged vegetation can produce either an increase or a decrease.

p.2904 l.13 Not sure if hypothesis 3 and 4 are defendable as high floods will submerge vegetation (e.g. agricultural land) and cause DTf to become DTw and fw to increase. Please discuss this.

- fw is constant by definition since is the fraction of "permanent" water bodies. This does not exclude that, in case of intense flooding, temporarily flooded areas show the same polarization difference. The algorithm is structured in order to consider this. As previously stated, hypothesis 4 was initially formulated by Sippel et al., 1994, but we overcome it in the present paper, since we estimate variations of water level in flooded wetland. We will clarify this in the text.

p.2904 All explanations referring to the radiative transfer model are rather vague (e.g. section 2.5.4). It is not clear to what extent this model is sensitive to its parameters and input data.

- The model was described in Section 2.4. Giving details is outside the objective of this paper. Then we refer to previous publications. The inputs of Table 2 are based on previous field work. In any case we will try to give a more detailed description.

p.2905 l.9 Is there any background on the assumption that the LAI is reduced linearly

HESSD

8, C2162–C2172, 2011

Interactive Comment

with increasing water levels? Please clarify the rationale behind this assumption.

- There is experimental fieldwork that shows that for this kind of marsh vegetation, LAI is uniformly distributed in height. See the references cited in Section 2.4.

p.2905 l.25 consisting p.2906 l.14 on six different dates p.2906 l.22 depend

- The corrections will be included in the text.

Section 2.5.1. The choice of threshold values is critical. I would expect that an increase of the backscattering coefficient by 1.5 db could also be due to an increase in surface soil moisture (mostly on bare soils, if there are any within the AOI). With this (simplistic) algorithm there is certainly a high risk to overestimate the fraction of flooded area.

- The problem of thresholding has been critically evaluated by considering four different criteria. Of course, making the threshold more severe reduces the flood fraction, but we obtain also a corresponding increase in water level. See Figures 3 and 4 at the end of this document. We believe that this is consistent, since setting a higher threshold is simply a change of the rule to define a pixel as flooded, and these results correspond well to this (Figure 4). The reasons for which we do not attribute an increase of sigma-0 to soil moisture have been discussed previously.

Section 2.5.1 Do you have any idea on the accuracy of the ENVISAT ASAR-derived ff estimate within wetlands. The observed increase in backscattering is arguably due to flooding under the canopy AND soil moisture. This should be clarified and discussed. What would be the effect of overestimating ff?

- As we stated, a validation at this scale is very difficult to perform. But since there is no bare soil in the area and the soil is normally wet, the increases of sigma-0 are related to increases in water level. See Figure 3 and 4 at the end of this document.

p. 2908 l.18 are in good agreement with

- The corrections will be included in the text.

HESSD

8, C2162–C2172, 2011

Interactive Comment

p.2908 l.12 you assume that DTnf and DTw are constant in time (values in Table 3). In this case I suggest indicating the observed values of DTnf and DTw at each acquisition, as this would better support this assumption.

- This is now discussed in more detail in the text. The values of deltaTnonflooded cannot be estimated from images, since no homogeneously covered non-flooded areas are present at this resolution. Therefore, values obtained from simulations were adopted.

p.2908 l.19 please provide some plot indicating the “good agreement” between model simulations and AMSR-E measured values for DTnf.

- A direct comparison between simulated and measured values of deltaTnf is not possible due to resolution issues. However, in the new version we compared measured deltaT for low water levels with simulated deltaT for WL=0. On average, the simulation corresponds to the measured values, but large dispersions are observed.

p.2909 l.11 should be Table 3

- The corrections will be included in the text.

Fig. 5 It would be interesting to provide more physical background on how brightness temperature varies with water level. Moreover, is there any way to indicate how reliable the lookup table DTff – water level depicted in Fig. 5 is? Would it be possible to add on the same plot values of actual water level and DTff measurements?

- The physical background of the trend of deltaTflooded with water level is provided by the physically based interaction model. An error budget for every part of the procedure and the look up table in particular will be included. Due to spatial resolution issues, no values of deltaTflooded can be obtained from images. Moreover, water level is monitored in the river, but not within the marshes. We use model simulations just for this reason.

p.2909 l.24 “Slope” not appropriate term to characterize curves in Fig.5 p.2910 l.17 edit

HESSD

8, C2162–C2172, 2011

Interactive Comment

sentence p.2911l.9 edit sentence p.2912 l.18 using

- The corrections will included in the text.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/8/C2162/2011/hessd-8-C2162-2011-supplement.pdf

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 2895, 2011.