Hydrol. Earth Syst. Sci. Discuss., 8, C900–C903, 2011
www.hydrol-earth-syst-sci-discuss.net/8/C900/2011/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Assimilating SAR-derived water level data into a hydraulic model: a case study" *by* L. Giustarini et al.

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Received and published: 14 April 2011

SAR-derived flood data are being utilized more and more to support hydraulic modelling and the paper by Giustarini et al. addresses relevant scientific questions in this field. The paper is also well written, and the results are sufficient to support the (valuable) conclusions. However, I have a number of minor comments that should be addressed and some recommendations. A first set of comments are about the model. I am totally aware that the paper does not focus on the calibration of hydraulic models, but some assumptions should be better described and supported (see points 1-3).

SPECIFIC COMMENTS

1. To calibrate the model, Giustarini et al. use contemporaneous measurements of

water levels and discharge. In principle, I think that this can be a good calibration strategy. However, these measurements were performed in the period 2001-2009 and I presume that the geometry of the river has changed over this period. Thus, this calibration data is expected to be affected by significant noise because of changes in the river geometry and, in particular, of the cease-to-flow level. This is evident by observing the two measurements in Pfaffenthal (Fig. 7) at hydrometric levels around 60cm when the measured discharge decreases for a higher hydrometric level. Given that the calibration exercise focused on the highest flow measurements, this noise might be negligible, provided that for river discharge values sufficiently higher than the bankfull discharge, differences in water stage due to changes of river geometry actually tend to vanish. This is experienced in many alluvial rivers where changes in the river geometry mainly occur in the main channel and therefore do not have a strong effect on the flood hydraulics when the floodplain gives a relevant contribution to the flow (Di Baldassarre and Claps, 2011). I wonder whether this is actually case of this reach of the Alzette River. Anyhow, this issue should be discussed.

2. It is not entirely clear how the calibration was performed as the paper only states that "the calibration aimed to reproduce the highest measurements of discharge". Anyhow, by considering the measurements in Fig. 7, I guess that the number of data used for model calibration is very limited (4, 8 or 12 points?). Thus, I think that using 4 parameters with such a low number of calibration data (4 different Manning's coefficients specifically adjusted in each cross section to fit a few points) might not be a parsimonious approach. In other words, I feel that this parameterization might lead to overfitting. More specifically, it is well known that a unparsimonious model tends to capture relatively much of the idiosyncratic information in the calibration data (i.e. noise; Wagenmakers, 2003; see also above comment). In fact, by simply adding parameters it is possible to fit almost everything (Fig. 7). However, such a model might make poor predictions as its parameter estimates tend to be affected by a relatively high uncertainty (Burnham and Anderson, 2002). I wonder whether the use a single Manning coefficient for the main channel would not be more appropriate for this mod-

elling exercise.

3. The floodplain Manning's coefficient is taken from Montanari et al. (2009). Nevertheless, as far as I can understand by reading the two papers, that calibration exercise was completely different from this one. In particular, that value of the floodplain Manning's coefficient was associated to a different channel Manning's coefficients. Now, it should be considered that many studies in the scientific literature have shown examples of parameter compensation: decreasing floodplain Manning's coefficients can be compensated by increasing channel Manning's coefficients (e.g. Hunter et al., 2006; Di Baldassarre et al., 2009). Thus, the use of the floodplain Manning's coefficient derived by Montanari et al. (2009) should be critically discussed and commented.

4. In my opinion, the example application (Alzette River) is functional to facilitate the description of the proposed procedure. However, given the current existing availability of SAR data and the actual resolution and revisit time, I feel that such a procedure is presently more suitable for larger rivers. Also, it might be worth noting that Schumann et al. (2010) recently demonstrated that globally and freely available space-borne data sets (SRTM, ENVISAT ASAR in WSM) can be used to approximate flood levels on large rivers. This potentially might allow such technology to be extended to data scarce areas and developing countries. Hence, I think that the paper would benefit by exploring and/or discussing the possibility to extent the method to larger rivers where globally and freely available remote sensing data can be used to derive water levels.

5. The abstract (page 2104, lines 4-5) and the introduction (page 2105, lines 4-6) mention that SAR-data "can be used for updating hydraulic models in near-real time". However, the paper does not include references to previous works (e.g. Di Baldassarre et al., 2009) nor clear evidence of this statement. I feel that a reference is also needed when LISFLOOD-FP is mentioned (e.g. Bates and De Roo, 2000).

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 2103, 2011.