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Hydrology and Earth System Sciences Discussions

Interactive comment on "Discharge estimation in a backwater affected meandering river" *by* H. Hidayat et al.

H. Hidayat et al.

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We would like to thank anonymous referee#3 for assessing our paper and for his/her comments about the manuscript. Replies to specific comments are given below.

Specific comments

Comment 1: Which reference water level is considered is not clear (I understand it is the lowest known water level). Moreover, notations are not consistent in some equations (Eq. 6, Eq. 8) where H is written instead of $H + \eta$.

Reply: Indeed, the reference water level is the lowest recorded water level. The clarification about the reference water level has been added to the text, and the notation has

been corrected.

Comment 2: Eq. 9: the right-hand term should be integrated over the width (β from 0 to 1). As visible in Fig.2, a large portion of the river width is not seen by the H-ADCP at both right and left sides. It seems that the main lobe hits the bed at n=170m, but side-lobe reflections are likely to occur at a shorter range. The effective measuring range should be indicated precisely. Nothing is said on how is computed the discharge in edge subsections without H-ADCP measurements. These subsections are large. Therefore, I don't know how the total discharge can be computed from Eq. 9 only.

Reply: The DSM relies on a linear relation between the specific discharge q and the total dischare Q. Obtaining Q by integrating q over width would require q to be available over the full width, which in turn would require the H-ADCP to cover the full river width. Since that will never be attained, for reasons of side lobes and main lobes hitting the surface or bed, as outlined by the reviewer, the DSM is appropriate in most applications. We have been rather brief in explaining these issues because there are two recent previous publications using Eq. 9 (Hoitink et al., 2009; Sassi et al., in press). Nevertheless, we explained Eq. 9 more elaborately in the revised ms. Indeed, more readers may have in mind that the H-ADCP range should cover the entire range.

Comment 3: The need for an amplification factor $f(\beta)$ is not justified. It appears to be a correction factor, accounting for errors in the DSM assumptions and/or errors in the H-ADCP measurements. Since the total width value used to normalize beta is not mentioned, it is difficult to position the results shown in Fig. 7 in the section shown in Fig. 2. However, the dramatic increase in f for $\beta > 0.8$ is consistent with a huge underestimation of HADCP velocity measurements due to acoustic reflections from the bed, and potentially also the free-surface. Eq.1 and the accompanying text ignore that acoustic reflections from the free-surface induce underestimated, usually near-zero, velocity measurements, which cannot be directly corrected (cf. e.g., Moore et al. 2010). Please add to Fig.7 the corresponding velocity (ADCP+HADCP) and intensity (HADCP) profiles, in order to clarify this issue.

Reply: Justification regarding the need of $f(\beta)$ is extensively discussed by Hoitink et al. (2009), in section 6 of that paper that is entirely dedicated to this issue. The total width value to normalize β is 270 m. The reviewer misinterprets f, by stating that it appears to be a correction factor, which is not the case. Fig. 7 is not based on H-ADCP data, as interpreted by the reviewer, but on conventional boat-mounted ADCP measurements. It shows how specific discharge q times width relates to total discharge Q. The decrease of q near the opposite bank is not dramatic, but exactly what can be expected near the shore. We have added the above to the revised ms, which improves the readability.

Comment 4: The index velocity method (IVM) is not applied correctly by the authors. First, the IVM lies on a usually linear fit of the section-averaged velocity against an index velocity, here defined as the average of all HADCP velocities. In their regression, the authors used the discharge instead of the section-averaged velocity, as stated p.2671 l.20 and shown in Fig. 8, and inconsistently with what is stated in Section 3.3. The discharge-index velocity relation is usually more complex than a linear function, due to the area-velocity relation included. Second, the IVM results would be certainly improved if only the first HADCP cells would be used, since the velocity measurements in the last cells are likely underestimated and affected by reflections from bed and free-surface. Last, the separation of 'cal' and 'val' discharge data used to assess both IVM and DSM methods is unclear (not explained, and 'val' campaigns are not introduced in the text). The quite constant Qivm/Qbs ratios reported in Table 2 indicate that the IVM is not properly calibrated and could be corrected, just as the DSM is corrected with the amplification factor.

Reply: As stated in the manuscript, we applied the standard IVM, being the most straightforward way of obtaining discharge from H-ADCP velocity data. Regressing the H-ADCP index velocity with cross-section averaged velocity, and then multiplying with cross-section, requires a slightly higher effort, and can be regarded as a step in the direction of our method. In the revised ms we have included results from this

additional approach. As mentioned previously, we exclude measurements from the first H-ADCP cells to avoid the possible influence of boat activity at the jetty. Figure 3 in the manuscript has shown clearly the separation between the 'cal' and 'val' datasets, by including the date when each survey was carried out. We use results from the 'cal' boat surveys to perform the calibration in the IVM. A sentence has been added to introduce the 'val' campaigns in the revised ms. Velocity underestimation in the last cells occurred only during extreme low flow conditions. The relatively constant Q_{ivm}/Q_{bs} ratios reported in Table 2 occurred due to the fact that the three validation surveys were carried out during almost similar conditions of medium to high flows.

Comment 5: While applying the Jones (1916) formula, it should be more accurate to compute the flood wave celerity as $dQ/dA = 1/b \times dQ/dd$ if $d = A/b$. With this correction, the computed stage-discharge relation may change significantly, however likely not enough to reproduce the observed scatter. Please give the value of bed slope, and rework the computation accordingly.

Reply: A similar comment was posted by Prof. Koussis previously. The corrected c value is 1.28, which is less than the c value of 1.66 used in the previous computation. Wave celerity, however, is not a very sensitive parameter in the Jones' formula and this correction causes only a slight change in the estimated discharge. We have included this correction on the corresponding subsection of the manuscript. The bed slope value is $^{-4}$ and has now been mentioned.

Comment 6: P. 2679 and in several other sections, hysteresis (due to transient flow effects) and variable backwater are not clearly distinguished, possibly misleading some readers. For instance, 'hysteretic' should be used only for transient flow effects, and highlighting some representative events with solid lines in Fig.10 would be helpful to reveal whether we are facing nice hysteresis loops or variable backwater trajectories. The scatter cloud does not provide information on such trends. The results of the Jones formula (once corrected as previously discussed) are the best way to show that the observed stage discharge relations cannot be explained by transient flow effects.

This should be stated more clearly.

Reply: Backwater effects cannot be isolated completely from nonlinear wave effects. As we argue the ms, variable backwater effects were likely to occur *within* hysteresis loops. An additional complication is the generation of subharmonics generated by river-tide interactions, as explained.

Comment 7: Instead of 'the invalidity of the kinematic wave assumption', the main problem is variable backwater effects.

Reply: Backwater effects render the kinematic wave assumption invalid.

Technical corrections

W and b are both used to denote the river width. Please choose only one notation.

Reply: in the revised ms the river width is consistently denoted by B.

The term MSf and the whole first sentence of page 2680 are not clear to me.

Reply: This last part of the paragraph explains another factor affecting discharge in the river reach. It is striking that tidal influence can reach the site, which is located 300 km upstream of the river mouth in the Mahakam delta. The tidal signal is clearly visible during low flows. MSf is, as indicated in the text, the oceanographic term for a fortnightly constituent of the tide, created by nonlinear interaction of the tides induced by the Moon and the Sun with the river discharge. We will offer a more elaborate explanation regarding this terminology and its influence on discharge in the revised ms.

effected ! affected (p.2681, l.2); meaning of 'a.o.'? (amongst others?)

Reply: The Correction has been carried out, and amongst others has been written in full.

References

Hoitink, A.J.F., Buschman, F.A., and Vermeulen, B.: Continuous measurements of discharge from a horizontal acoustic Doppler current profiler in a tidal river, Water Resour. Res., 45, W11406, doi:10.1029/2009WR007791, 2009.

Moore, S.A., Le Coz, J., Hurther, D., Paquier, A.: Backscattered Intensity Profiles from Horizontal Acoustic Doppler Current Profilers, in River Flow 2010, Braunschweig, Germany, 2010.

Tsai, C. W.: Flood routing in mild-sloped rivers – wave characteristics and downstream backwater effect, J. Hydrol., 308, 151—167, 2005.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 2667, 2011.