Reply to Comments by:

REVIEWER 1

The manuscript presents an application of entropy theory to determine the discharge at a river section from measurements of the surface velocity. The key features of the manuscript are (i) comparing the discharge estimates when a transverse profile of surface velocity is entirely known and when only a single-point value of surface velocity is known and (ii) considering sections where the flow distribution is significantly disturbed by interfering structures like bridges.

The manuscript is quite interesting but I feel that some revision could strengthen it and make the method more appealing for readers. I have some major concerns with this manuscript:

RE: We thank the reviewer for appreciating the paper and for the constructive criticism and comments, which allow us to improve the paper.

MAJOR COMMENTS

(1) Section 2.5, entitled "Entropy theory", provides some equations but no concept at all. Which is the grounding principle of this method? Which is the physical meaning of M and phi(M) or, in other words, what do these parameters parameterize? No need to rewrite referenced papers, obviously, but providing some conceptual ground would be needed to understand the description and interpret later findings (for example, at line 234 it is mentioned that the disturbance by the bridge reduces the entropy parameter; why? Which is the physical interpretation?).

RE: We agree about the need of providing some basic concepts of the entropy model and of clarifying the physical concepts of the involved parameters. We enriched the text of Sect. 2.5 with some basic concepts on the entropy theory, the physical meaning of the entropy function, $\phi(M)$, which is the ratio of the average to maximum flow velocity, of the associated entropy parameter, M, and their typical behaviour in rivers as outlined by previous studies. For example, the disturbances produced by the bridge act to increase the maximum velocity with respect to its average value, i.e., to reduce the entropic function $\phi(M)$ which is defined as the ratio of average to maximum velocity in the cross-section. This is coherent with the fact the localized geometrical variations can produce local accelerations and increase the unevenness of the cross-sectional velocity distribution.

(2) The authors should better describe how they use data and to do what. My understanding is that they rely on surface velocity measurements by ADCP and on the results of the 3D simulations. The ADCP measurements are used to "validate the numerical models" (line 136), while the entropy theory is applied to the simulated flow fields. For the latter, the procedure is: (i) determining the entropy parameters phi(M) and M based on the simulated velocity distribution in a cross section (obtaining the values listed in Tab 2 and plotted in Fig 3); (ii) determining a velocity distribution in the cross section based on the surface velocity from the simulations (entire profile or single-point); (iii) integrating the obtained velocity distribution; and (iv) compare the discharge with that used as an upstream boundary condition in the simulations. Now, steps (i) and (ii) sound like creating a loop, more or less like when the same data are used for calibration/training and validation. It is not completely so, since a bulk estimate of an entropy parameter is later used to determine a velocity profile at every vertical, but some self-dependence should be present. Can the authors comment on this issue? Or, I may have misunderstood the procedure, that would need more explanation.

RE: We admit that the use of different data was not clearly described in the paper, and we apologize for that. We clarified the point at the end of Section 2.5 (and in other part of the text) by better explaining how the entropic model is applied, which data is used as input and which data only to check the accuracy of the outputs. We are confident that this allows to better understand the use of data and that there are no loops in the adopted procedure.

Basically, the entropic distribution of the cross-sectional velocity depends the maximum velocity (for each vertical or for the whole cross-section), on the depth at which the maximum velocity occurs (i.e., the so-called dip), and on the entropic parameter, M, which has a one-to-one relationship with the entropic function, $\phi(M)$. The entropic function, $\phi(M)$, has a clear physical meaning, as it is the ratio of the average to maximum velocity in the cross-section. In our analysis, we first computed $\phi(M)_{CFD}$ from the cross-sectional velocity distributions that have been measured and simulated with the 3D-CFD model, to check how the physics-based estimates of $\phi(M)$ vary in the considered cross-sections. To apply the entropy model, $\phi(M)$ and M were estimated using literature formulae and used in an iterative procedure that is independent of the values $\phi(M)_{CFD}$ obtained from the 3D-CFD model, which avoided creating a loop. Indeed, of the flow fields computed by the 3D-CFD model, we used the surface velocity at the four cross-sections (either their river-wide distribution or only the maximum value) as input data for the entropy model, and then we used the 3D-CFD cross-sectional velocity distributions as a benchmark for the velocity distributions computed with the entropy models. We admit that these aspects were not clearly explained in the previous text.

(3) It is not clear to me how this method could be applied at a section where the entropy parameter is not known, starting just from a profile of surface velocity. My impression is that, in such a case, a tentative value of the entropy parameter should be used. How estimated? Expert judgement leading to sound values, as we normally do for roughness? I note that also the iterative procedure described at lines 192 and following "can be applied for sites with a given phi(M)". Related to this issue, I note that an estimated discharge sounds quite sensitive to the entropy parameter, as the two quantities are linearly dependent based on eq. (2). The problem is the same that affects discharge determination based on large-scale PIV (e.g., 10.1029/2008WR006950, 10.1016/j.jhydrol.2010.05.049) where a coefficient is used to pass from surface to depth-averaged velocity. The coefficient is not known, but most references use 0.85 as a suitable value. In the present context, one might use phi(M) = 0.72 for "undisturbed" flows (average of values for -50 and +200 in Tab 2), but it will be hard to have a robust indication for disturbed flows.

RE: The issues raised in this comment are indeed related to the unclear aspects mentioned in the previous comments. We admit that the procedure used to apply the entropy model was not described with due clarity. The sentence "can be applied for sites with a given phi(M)" was wrong as de dip, h, is computed based on formula (now added to the text) derive in laboratory experiments that does not depend on phi(M). However, for cross-sections in which only the surface velocity is known (and there is not previous information on the cross-sectional velocity distribution), an iterative method is used in which the first guess for $\phi(M)$, computed with a standard formula, allows determining the first-iteration velocity distribution. Then, based on this cross-sectional velocity distribution, $\phi(M)$ and, in turn, M, are recomputed. The procedure is repeated until the difference between successive values of $\phi(M)$ becomes less than a small threshold value, e.g., 0.01. Moramarco et al. (2017) demonstrated that the vertical distribution of the velocity based on the Entropy method is generally more accurate than that estimated based on the velocity index method. Furthermore, while the velocity index model gives only the average velocity, the entropy approach estimates the whole cross-sectional distribution of the flow velocity, which in turn allows estimating the other quantities of interest, such as the bed shear stress, etc.

(4) I would suggest to carefully reconsider the use of "at" river bridges in the title. My initial interpretation was that the focus of the paper would be on sections immediately upstream or downstream of the bridge, and I think that most hydraulicians would have done the same. Instead, the paper considers sections in the vicinity of the bridge but at some distance from it (some/several widths). This is quite important for applying the method using monitoring data since, as remarked by the authors, instrumentation is generally mounted on the bridge.

RE: We understand the comment and acknowledge that the cross-section we used in the analysis are not located exactly at the bridge. The sections that are closest to the bridge are 50 m upstream and downstream of the bridge, a distance which is about $0.45B$, with B the width of the river at the bridge section. These sections are indeed very close to the bridge, particularly considering that, while the water level sensors measure perpendicularly, the remote sensors for surface velocity (such as radar, Large Scale PIV, etc.) have their field of view located some tens of meter upstream or downstream of the bridge. Similarly, when a current meter is operated from a bridge, the drag force moves the current meter downstream at a distance that depends on the height of the bridge with respect to the free-surface elevation and on the drag force itself. Finally, note that the flow fields predicted by the CFD model show that the velocity distribution varies abruptly at the upstream edge of the bridge (flow contraction), and smoothly in the tens of meter upstream and downstream of the bridge. Accordingly, for the purpose of the study, we remain convinced that the particular choice of looking at cross-sections located 50 m upstream and downstream of the bridge can still be regarded as sections "at" the bridge. We are reluctant to use "in the vicinity" because the title is already quite long (and it was extended to include "velocity distribution" according to the suggestion by Reviewer #2). We added some text at the beginning of Sect. 3 to explain this issue:

> "The sections just upstream and downstream of the bridge are located at a distance of about $0.45B$ from the bridge, with B the width of the river at the bridge section. This is a short distance, particularly considering that the remote sensors for surface velocity (such as radar, Large Scale PIV, etc.) have their field of view located some tens of meter upstream or downstream of the bridge. The sections far downstream are considered to assess how far the flow field is affected by the presence of the bridge."

Finally, the fact that we also considered two cross-sections further downstream of the bridge is only meant to assess how the disturbances generated by the bridge vary in space. The core of the study is an attempt to answer the question: "how can we estimate the discharge with the entropic theory and measures carried out from river bridges?", which we believe is well reflected by the modified title.

<u>MINOR COMMENTS</u> (listed by line)

16: I think that mentioning explicitly that 12 sample applications are considered (3 flow rates by 4 sections) would make the manuscript findings sound more robust.

RE: We agree. The Abstract was modified accordingly:

> "A total of 12 samples, including three different flow conditions for four cross-sections, one upstream and three downstream of the bridge, are considered. ~~Different flow conditions and cross-sections, either upstream and downstream of the bridge, are considered.~~"

17: not sure that the word "safely" is the appropriate one to state that the method is satisfactorily accurate.

RE: We agree. We changed "safely" with "reliably".

50-68: I find these lines a bit hard to follow, due to the lack of a clear purpose. To me, the point is that one would like to determine Q based on its definition, that is integral(v)dA. Since the cross-sectional distribution of velocity is unknown, a method to infer the cross-sectional velocity distribution from other data (that are in the end surface velocities) is sought. I would recommend to use this line of thought to revise these lines.

RE: Thank you for the feedback. We reworded and re-organized most parts of the Introduction, that was actually a bit confused.

70: the method based on a ratio between surface and depth-averaged velocities could be mentioned as an alternative one (some references are found in major comment 3).

RE: We mentioned the method indicated by the reviewer indicating the advantages of the proposed entropy approach. Now the text reads:

> "One advantage of the entropy approach is providing the complete cross-sectional distribution of velocity. By contrast, other indirect methods for estimating flow discharge only compute the depth-averaged value from the surface velocities at subsections using a fixed reduction coefficient (e.g., Le Coz et al., 2010)."

76: what is meant with "weak" gauging site?

RE: We intended a gauging site with insufficient data to obtain reliable estimates of the flow discharge. This part of the text has been deleted to improve the readability of the Introduction.

107: if the river is characterized by high sediment transport, one can expect relevant morphologic changes. The simulations carried on in this work are with a fixed bed and always the same geometry; this is not a problem for the present manuscript but morphologic changes complicate the business in case of future application of the method. In the present version of the manuscript this is almost overlooked, apart from a quick mention at the end of the Conclusions. Few more lines on the issue would be an important addition.

RE: We agree in that morphological changes can really complicate the business in many practical cases. According to the suggestion, we changed the Conclusions to remark the possible limitations of assuming a fixed bed. Now the text reads:

> "A main limitation of the present methodological approach relies in the assumption of fixed bed in both the CFD analysis and the application of the entropic model. In natural rivers, bed scouring during sever flood events and the ensuing formation of local deposits, especially close to in-stream structures such as bridges, can alter the bathymetry and, in turn, the velocity distribution and the discharge estimates. In case of movable bed and absence of protection measures (e.g., riprap or bed sills), the uncertainty associated to the local bed mobility has to be evaluated with due care.
>
> Future research ~~will include the analysis of stage dependent variations of cross-sectional velocity distribution, particularly in case of compound cross-sections that are typical of natural rivers. M~~ on more complex scenarios that still need a comprehensive

assessment, and which could largely benefit from physics-based numerical modelling, will include the case of mobile beds, ~~in which the geometrical variability occurring at the passage of floods adds uncertainty to the discharge estimation~~ and the analysis of stage-dependent variations of cross-sectional velocity distribution, particularly in case of compound cross-sections that are typical of natural rivers."

168: the events could be better described in terms of (i) flow over the sediment bar, (ii) pressurized flow below the arches, and (iii) bridge overflow. Later in the manuscript the reader will understand that only for the strongest event there was incipient pressurization of flow below the arches. It would be probably better to add details here.

RE: We added some details to the text, which now reads:

"In all the three flow conditions, water flowed in the main channel and over the sediment bars that are dry in the low flow condition of Figure 1b,d. During the most severe flood of 2012, water flowed on the floodplains adjacent to the main river and caused the incipient pressurization of flow below the bridge arches ~~water flowed also through the floodplains adjacent to the main river channel.~~"

173: is it possible to give some value of return period for these events?

RE: Yes. The return period associated to the three flood events are about 200 years for the 2012 event, 2 years for the 2019 event, and 1 year for the 2022 event. The information has been added to Table 1.

Tab 1: the discharge values come, I guess, from rating curves mentioned at line 133. Is it so?

RE: Yes. In the revised manuscript we have clarified this point.

Eq. (2) and (3): this is the first time M and phi(M) appear, but they are not given a name.

RE: Thank you for noting. Now the entropic function, $\phi(M)$, and the entropic parameter, M, are properly defined and referenced throughout the text.

Tab 2: please indicate how the values of phi(M) and M were obtained. From (3) and (2), respectively?

RE: Now it is clarified that the value reported in Table 2 of the previous version were obtained from the 3D-CFD velocity distributions. In the revised version, we added the values computed with the formula used to initialize the iterative procedure used in the entropy model. We also clarified how these values are computed, i.e., $\phi(M)$ from a formula for the first guess, then as the ratio of maximum to average velocity, and M from $\phi(M)$ according to Eq. (2).

236: I would say this is true also for the 2019 event.

RE: Thank you for noting. The text was modified according to the suggestion.

294: I think that "accuracy" would be better than "precision" here. Same at line 366.

RE: We agree and substituted "precision" with "accuracy".

321: actually, in Tab 4 the errors for elliptic are larger than those for parabolic.

RE: We agree that the total discharge estimation appears more accurate for the PSD rather than ESD. However, the sentence was a comment on the cross-sectional distribution of the velocity for the undisturbed sections reported in the plots. This point is better specified in the revised version of the manuscript.

354: like the one mounted on this bridge (I mean the radar sensor).

RE: We added this specification to the revised text.