Authors's Response to

1st: Hydrol. Earth Syst. Sci. Discuss., 11, C1919–C1922, 2014

G. Benito (Referee)
Received and published: 19 June 2014

The paper shows interesting details on how the authors have been estimating flood discharges from historical land marks. Not going into details regarding other ways to infer historical flood discharges, it is an interesting paper and deserves publication. The paper is very much focus on the antecedent bibliography on central European cases, and ignores some previous work done in other parts of Europe. Moreover, some confusion on the meaning of palaeofloods and historical floods should be improved, according to comments given below. It is important that the authors also mention the high uncertainties of using Manning equation which assumes uniform flow, which is not common during flood flows.

- As explained in detail in the published reply to the referee's comment, an explanation on why and how we differ between palaeofloods and historic floods is given on page 5464 line 26 of the manuscript, the references given by the refee below are mentioned.

- The term "uniform" is added in the context of the explanation (5467 line 16 and "Uniform flow with each unit is assumed" is added on page 5468 line 6.); further comments were given in the reply already.

Specific comments:

Page 5464 line 23 "Previous floods can be divided into historic and palaeofloods. The difference is based on the duration of historic times with handed down historic documents or descriptions while palaeoflood events took place in prehistoric times." This statement is uncertain. Although many palaoefloods occurred during prehistoric times, palaeofloods refers to the type of evidence used to identify or reconstruct the floods, i.e. palaeofloods used geological and botanical evidences and historical/documentary floods used written or observed descriptions. Refer to Brazdil et al., 2006 "Historical hydrology for studying flood risk in Europe" HSJ 51, 739-764. Read page 742. – cf. above

Page 5475. line 1. delete e.g. – e.g. should remain to illustrate mentioning the condition are only an example while elsewhere the threshold might be another one.

Page 5475. Line 2 to 3. Same comment that above. There is no a time transfer between palaeofloods and historical floods, and in fact there are overlapping in many instances (e.g. Benito et al. 2010. paper in Global and Planetary Change). – cf. above

Page 5475. Line 9. "palaeofloods … only be estimated using relative chronologies or physical based dating techniques" Palaeofloods may also be dated using numerical dating (radiometric techniques) typically by radiocarbon and optically stimulated luminescence (OSL). – "like radiocarbon or optically stimulated luminescence" added to illustrate the meaing of physical dating techniques better.

Page 5475. Line 11. Sedimentary records in lakes may also give an annual resolution. Check Corella et al., 2014 QSR. – varves are additionally mentioned and the reference added.

Page 5475. Line 17 “their relation to recent and near future floods conditions are more obvious than for palaeofloods from geological times.” It is fine you address the reconstruction of historical floods, but as indicated previously, palaeofloods may refer to historic and even modern floods, so not need for this statement. Check for instance paper by Thorndycraft et al 2005 were many of the palaeofloods corresponds to 20th flooding.
Thorndycraft, V., Benito, G., Rico, M., Sopeña, A., Sánchez, Y and Casas, M. (2005). A long-term flood discharge record derived from slackwater flood deposits of the Llobregat River, NE Spain. Journal of Hydrology , 313 (1-2), 16-31. – cf. above

Page 5466. Line 15. I would like to draw your attention regarding the paper by Benito et al. 2003 in the Tagus River, that is probably one of the first papers in Europe deriving peak discharges from historic events based on flood level marks and documentary descriptions combined with step-backwater hydraulic modelling, in a systematic way. The paper provides discharge estimates of 110 historic floods (some started in AD1113) in four sites along the Tagus River. I suggest referring to it.
Benito, G., Díez-Herrero, A., de Villalta, M. (2003). Magnitude and frequency of flooding in the Tagus river (Central Spain) over the last millennium. Climatic Change, 58, 171-192. – reference is added.

Page 5467. Line 1 and followings. I am sorry to say that Manning equation assumes uniform flow under normal conditions (read Chow “Open channel hydraulics” book in page 91-92) that is, if there are not flood flows or markedly varied flows caused by channel irregularities. As indicated by Chow (1959) the results of applying the uniform-flow formula to a natural stream are very approximate since the flow conditions is subject to high uncertainty factors, since normal flow rarely occurs in natural channels. You should indicate these facts in the text or discuss about this. – cf. above: “uniform” added page 5467 line 16 and “Uniform flow with each unit is assumed” is added on page 5468 line 6.

Page 5467. Lines 23-24 “According to own experiences and generally speaking, the units of settled areas, the river channel and the floodplain provide obviously different hydraulic roughness”.
This sentence is strange because is not a problem of experience, it is just a fact that floodplains and river channels have by definition different hydraulic roughness. Even within the floodplain, this may be divided in different sub-areas with different roughness. This is state later in the following lines. – “According to own experiences and” is deleted.

Page 5469. Line 1. change “meter” by “metres above sea level” – done.
Lines 1 to 4. The sentence is too long and complicated. I would suggest to short it or to produce two separate sentences. - done

Page 5469. Line 5. The meaning of this sentence is not clear. What do you mean by the “principle units”?? – “principle units” is changed to “separated units”

Page 5485. Figure 3. Scenario 1 and 2 should be written with “c” – done, other mistakes are also corrected.

Authors's Response to

2nd :
Hydrol. Earth Syst. Sci. Discuss., 11, C2498–C2499, 2014

Note the detailed statements submitted before as reply on the comments of the reviewer. To explain what we have done, they are pasted here in again.

Beyond spelling mistakes, most questions mentioned by the referee rather seem to go into further details than focusing on uncertainties within the manuscript itself. Probably it is useful if the editor decides if and which question should result in a specific improvement of the manuscript in addition to the explanations made and references given below and be published as reply on the referee's comments.

Anonymous Referee #2
Received and published: 15 July 2014
An excellent and very impressive presentation of method, how to determine very variable merit using very simple method and approach. The methodology seems to me to be based on extreme expertise and experience of the author, and hardly can be applied generally by someone else. I understand the paper as presentation of very deep knowledge and abilities of the authors, but can not imagine replication of the method by another engineer. I definitely recommend paper for publication as really excellent example of applicability extremely simple method based on deep knowledge and experience. However, I have a few questions or comments:

- Please revise English spelling at Figure 3 – SZENARIO - done

- Page 5468 row around 10 – can you specify, how to define or select suitable scenarios and how many of them shall be sufficient number ?
If different scenarios are required at all depends if any distinct (!) uncertainty arises, e.g. two different peak flood levels for the same event at the same place are handed down. As mentioned in the text, the problem whether remnants of a destroyed stone bridge filled the locally narrow river channel and therefore significantly decreased the cross-section area illustrates another example requiring consideration of different scenarios. If they are actually needed and how many cannot be answered in general but depends of evidence at specific locations. Note, that in worst case numerous scenarios might be considered, but if no contrasting reliable data exist or case studies are obviously needed, no differentiations are necessary (and useful) at all. If case studies are obviously needed and how they can be quantified is the decision of the investigator in each individual case study (like for any modelling approaches in general, I think). As these aspects seem to be sufficiently explained in the manuscript already (p. 5468 l. 7-15), no modifications seem to be necessary

- Page 5468, row 25 – can you describe, document or cite, how to backtrack the layers (which method do you use, or which shall be used ?)
Different layers of sediment or urban debris in floodplains reducing the cross-section area can be differed based on previous archaeological or soil-scientific studies. As mentioned in the text, the previous studies might be used to get information on the age, thickness and expansion of cultural layers. Further details depend on the individual lo-

cations, e.g. filled river channels for the expansion of settlements or shore stabilisation to ease harbour activities. As these aspects seem to be sufficiently explained in the manuscript already (p. 5468 l. 20-28), no modifications seem to be necessary.

- Page 5470, rows 10 – 20 – there would be better to structure the text into bullets or paragraphs. This section has more character of narration, than highly expert text
Sorry, but we cannot follow how and why the text explaining principally the potential influence of the natural incision rate of a river should be transferred in its layout towards a space-consuming list. Also an (unqualified?) narrative character cannot be identified as we are just explaining the resulting minor river channel incision amount since historic times for an unrealistic high annual incision rate. Background are frequent comments during oral presentations of the quoted previous key studies that natural incision of the river channel (and accumulation on the floodplain) should result in significant errors of the historical cross-section area, which is shown to be less significant. As these aspects seem to be sufficiently explained in the manuscript already (p. 5470 l. 5-20), no modifications seem to be necessary.

- Page 5472, rows around 20 – this is very difficult even recently, but proper estimation in historic conditions needs extreme high experience and expertise. My compliment to this.
The quantification of roughness elements is based on previous experiences gained by other scientists published in the mentioned references. It might appear challenging to quantify the several parameters involved for historic times, but by a closer look (into the references mentioned above) the quantitative variation is less than it might be expected. Note further on, that in the context here, the roughness elements n1-n5 are of different importance for the different units of the cross-section area. As further details require lists that are rather space-consuming we can only refer a) to the quoted key studies with all variations listed in detail and b) the quoted reference publications on roughness quantification in general. As these aspects seem to be sufficiently explained in the manuscript already (p. 5472 l. 4 – p. 5473 l. 8), no modifications seem to be necessary.

– Page 5475, rows around 20 – do I understand well, tham Qmax and Qmin are limits of estimations ? And Qp is the mean ?. If so, what then is gauge data Qgauge in table 1 ? Are they historic measured data ? And what is their reliability ?
As illustrated in Fig. 3 and explained in the text (p. 5473 l. 7f), Qmin and Qmax result of maximum respectively minimum values of hydraulic roughness values of n. To be able to deal with a distinct single value for comparisons, a plausible (occasionally mean) value of n is estimated resulting in Qp. Due to algebraic reasons, Qp is not necessarily the mean value between Qmin and Qmax. As explained on p. 5475 l. 13-14, the estimated Qp-values are derived by the application of the approach on water levels of recent flood events which are compared with gauge data of the recent events to validate the reliability of the Qp-data in general. The reliability of recent gauge data cannot be quantified (by us) in general due to the reasons explained p. 5475 l. 23-25. Therefore, the gauge data are taken for “real” and representative. As these aspects seem to be sufficiently explained in the manuscript already (p. 5473 l. 19-22 and p. 5475 l. 13-14), no modifications seem to be necessary.

- Page 5476, row 6 – accuracy +- 10 % is extremely high and not many recent floods is estimated with such accuracy, due to uncertainties around shape for river bottom, roughness, flow velocities in individual parts of cross sections. Are you sure, you can reach such excellent results ?

As explained on p. 5475 l. 13-14 and table 1 (head of the last column) the “accuracy” is the difference between estimated Qp-data and measured Qgauge-data of recent flood events expressed in %. Note, that we are not talking about accuracy as for historic flood events no related “real” value is available and alternative approaches only can provide other estimations. As mentioned above, gauge data of recent floods are considered to be representative even if they contain errors (p. 5475 l. 23-25). (Unfortunately,) The excellence of our results has to be put into perspective. As these aspects seem to be sufficiently explained in the manuscript already, no modifications seem to be necessary