Hydrol. Earth Syst. Sci. Discuss., 10, C222–C227, 2013
www.hydrol-earth-syst-sci-discuss.net/10/C222/2013/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Sedimentation monitoring including uncertainty analysis in complex floodplains: a case study in the Mekong Delta" *by* N. V. Manh et al.

N. Gratiot (Referee)

nicolas.gratiot@ird.fr

Received and published: 5 March 2013

Review HESSD Sedimentation monitoring including uncertainty analysis in complex ïňĆoodplains: a case study in the Mekong Delta N. V. Manh, B. Merz, and H. Apel

The present paper addresses the problem of sedimentation monitoring in deltas. It is based on an intensive field campaign, during which 450 sediment traps were distributed strategically in the complex floodplains of the Mekong delta. At the end of the field campaign, 171 traps were recovered and various water and sediment properties were measured. This large scale campaign thus provides an unprecedented dataset spatially distributed over a wide delta.

The work follows two complementary objectives: (i) to propose a methodology to monitor sedimentation and evaluate the trustworthiness of sediment traps (ii) to assess the pattern of sedimentation in the Mekong delta, which is known as the most complex channel network in the world. These two objectives are of a broad international interest and the paper could potentially provide a good piece of work. However, in its present state the paper fails in reaching fully the two objectives:

(i) To address the first objective, the authors have combined some laboratory investigations with statistical analysis (quantification of individual errors, propagation and quantification of the overall uncertainty). The methodology proposed is scientifically sounding but the number of runs performed in the laboratory and the number of samples in each field clusters are very limited. This greatly weakens the robustness of the approach. Concerning the laboratory measurements, they do not seem to present any technical difficulties and it is somehow surprising that the authors did not conduct more runs. About 30 runs would be sufficient to have a statistically significant estimation of the loss of sediment from submerged traps. The evaluation of the deposition uncertainty through statistical characterisation is more critical. The authors underline the need of characterizing clearly small and large scales variabilities as well as their associated errors. Unfortunately the sampling strategy is not correct to apply the chosen statistical method. The authors propose to generate Probability Density Functions from two to three individual values. This number of individual samples is clearly insufficient to get robust PDFs estimates. The way the authors are justifying this strategy (lines 20-25 p336) is not really convincing. While the authors have an important number of sediment traps at their disposal (171), I am quite sure they could propose alternative strategies which would be better. Maybe the authors should focus their approach on the characterization of the uncertainties by functional compartments, as discussed in some paragraphs. The lack of statistical significance discussed previously as some direct impact on sections 5 and 6 and limits the relevance of the deduced interpretations/conclusions.

(ii) The second objective concerns the spatial distribution of sedimentation in the Mekong delta. At the beginning of the paper, the reader expects to obtain a quantification of the sedimentation in the Mekong delta. Because of the very high variability at small scales, this goal can not be reach. The discussion of spatial pattern is thus reduced to the presentation of some results for three sites chosen among the twelve sites monitored. This is quite disappointing and finally, the paper does not provide a clear strategy of monitoring, as initially expected (minimal number of traps per sites and/or functional zones, etc.). I am convinced that the paper as a good potential, but in its present stage, some major modifications regarding the statistical approach and the structure of the paper should be addressed.

Please, find here bellow detailed suggestions and comments:

327-22: epistemic, are you sure it is appropriate, isn't endemic?

327-25: I understand clearly that mat trap can be interesting for quality analysis because you collect some material. I am not convinced of the usefulness of this technique to quantify the sedimentation (can not capture the cycles of erosion, deposition ; can be saturated if sediment deposit exceed one to two centimetres, etc.). Do you have some experiences/references on this point? Could you comment?

328-1-5: Not that Altus systems have been deployed in estuarine areas and provided some quantified information on sedimentation and erosion. Maybe you should add some references on this technique.

328-19: not found in the reference section

328 – 27: a-1, all along the document you use this. I think that y-1 is more appropriate.

330-26 "The selected sites have to be distributed the main floodplains in the MD" unclear for me. A word is missing?

331-8: Hung 2013b, if you intend to resubmit the paper, I would be please to have a copy of the recent publications of your group (and the submitted publications).

331: probably here, you could indicate in the texte the number of traps collected.

331-24: did you weight all traps individually? This could be potentially a source of error.

333-10: you speak about 161 traps in the text and 171 in the table.

333-25: 500g. Please, also give all weight in g.cm-2 or in mm of deposit. How long did you dry the traps? For hundreds of grams of material, I guess it can take quite a long time?

334-8: how do you define the outliers? Depending on your choice for the outliers, Fig.4 can be very different no?

334-12-17: The text and Fig.5 are not very clear. The figure contains a lot of information that could be synthesized to get the message clearer. I expect that this is the section where you discuss the variability from various spatial scales and compartments.

334:22: Personally I do not see any trend for CV with the increase of the deposition mass.

336:1-2: As nutrients are mainly fixed on clays and silts, it looks strange to have no correlation with sand content (higher sand content, lower nutrient content).

Section 5.1: It would be far better to have much more runs. Maybe, you could express the mass in link with the depth of sediment deposition. When it reach 3cm, you reach the thickness of the traps!

Section 5.2: As already indicated in the general content, it has no sense to run some pdfs functions deduced from 2 or 3 samples.

337-5: normal distribution: same comment than previously! If I remember correctly my statistical courses, a pdf need about 30 points to be statistically relevant and stable.

337-8: not markedly skewed. How do you remove the outliers? If you consider all the points, it becomes skewed.

338-19: Once again, how many point do you consider to obtain your pdf?

339-22: flocculation can strongly modify your evaluation of the proportion of clays, silts and sand. What you measure and discuss in the paper is the effective/aggregated size and not the absolute/dispersed one. This needs to be clear for the reader.

341-10-15. Unclear

341-16-17. I believe that errors can even be higher than these estimates

341-21. step change. It is not rigorously a step, but an inflexion with a change of slope.

341-28. Sand highest uncertainty. Maybe in link with the flocculation processes.

342-12. When you estimate the deposition thickness how do you proceed? What is the density of sediment you are considering?

345-4-5. Please add some errors: X+-Y

345-10-14. During the interpretation, you should remind that traps are not reproducing the cycles of erosion and thus can diverge from the observed annual sedimentation

345-15. were monitored instead of weres.

345-17. that lead to completely

Table 2. As you use Robinson pipette technique to estimate grain size, aggregation will shift your results to higher grain size.

Table 3. Sand in %?

Table 4. Please remember that it is per year

In general, I believe you have too many figures which are not always clear.

Fig.1. The complete watershed, delimited in purple appears to be separated in two subparts (north and south). Why this delimitation?

Fig.1. Your legend considers altitude up to 12m ; the 4-12 m is beyond the range of observed values and should be removed.

Fig4. Define outliers. On the right axis, what means +1.5IQR?

Fig5. Coeff. Of Variation CV. I do not find this figure clear.

Fig6. I do not understand how you designed your laboratory tests: few points, not regularly distributed?

Fig.10. Here you assume no SD for the nutrient, am I wrong?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 325, 2013.