

***Interactive comment on “Inside the hydro-physics
processes at the plunge point location: an
analysis by satellite and in situ data” by
A. T. Assireu et al.***

Anonymous Referee #2

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The article presents a nice set of in-situ data along with multi-spectral satellite imagery of a large reservoir exposed to significant fluvial discharge. The study is focused on dynamics of the river plume (which may either be confined to the surface layer or form an underflow, depending on the density difference) and its role in sediment transport and mixing of the water body. The topic is definitely of theoretical and practical importance, and I do believe that the paper is interesting. However, in my opinion, rather serious revisions are imperative before publishing it.

General comments:

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1) The conclusions, as formulated, are not fully justified by the presented observational evidence and discussion. The authors affirm that the “Kelvin-Helmholtz instability was the main mixing mechanism” [in the wet season], but it is not clear why they are so sure. No quantitative assessment of this mechanism compared to other ones, such as wind mixing, internal waves, etc, is given in the paper (even though the data available to the authors apparently may allow such an analysis). The only argument of the author in support of their statement is a simple Richardson number based criterion (p. 1204, line 27). The latter only suggests that the KH instability is feasible, but does not mean, however, that it is the principal mixing mechanism. The other conclusion that “strong turbulence introduced by high winds and surface cooling were predominant” [during dry season] also needs a more elaborated justification.

2) Not all of the data that the authors claim to have used in their analysis are adequately represented in the article. For instance, it would be very interesting to look at the velocity data from drifters released in the reservoir, but there is no figure or discussion on this matter, and it is not even clear what these data were used for and how (perhaps, to obtain estimate for U_2 in Section 3.2?). On the contrary, some of the figures look unnecessary, for example, the long-wave and short-wave radiation series (Fig. 2, bottom panel) or pH profiles.

Specific comments:

- 1) The first word in the title should be omitted.
- 2) Section 2.1 is entitled “Field site and measurements” – and yet no measurements are described in this particular section.
- 3) The choice of numerical value for drag coefficient C_d in Eq. (5) and its applicability to these specific conditions should be explained.
- 4) Maybe Fig. 5 should include a zoom of “estuarine” area. In the present form, the images are too small to perceive details of the inflow.

5) Page 1200, line 17 – What is meant by “convergence zone”? Please explain.

6) The paper reads well, but still there are some minor language problems scattered throughout the manuscript, so some text editing would be useful.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 1193, 2011.

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