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Hydrology and Earth System Sciences Discussions

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Interactive Comment

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 #1

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According to the title of the paper, the reader is expected to be able to delve deeper into the physical processes at the plunge point of a buoyant flow entering a reservoir. However, after reading the abstract and the introduction, it seems clear that this is not the main topic of the paper. At the end of the introduction we read: "The purpose of this paper is to investigate both the potentiality and applicability of the use of thermal and visible satellite instruments to monitor the density difference between the water body and the inflowing water (plunge point location)". The idea seemed interesting to this referee although it is difficult to evaluate its novelty considering that the authors do not express clearly the state of the art of the topic, writing only the sentence: “There have been relatively few or absence of studies about the use of remote sensing to study the density currents in reservoirs”. Reading further it appears that the “Results and Discussion” section is divided into: a) thermal structure, b) river effects in thermal structure and Kelvin-Helmholtz instability, and c) analysis of a plunging underflow. There is no in-depth study of the potential of the use of satellite data to locate the plunge point and only two examples are discussed. Finally in the conclusion (P1207L1-15) there are two conclusions related to satellite data and two to the hydrodynamics of the reservoir (not to the inflow). Accordingly, the paper would benefit a lot from a more focused approach responding to the real aim (or aims) of the work.

The contents of the paper should be strengthened. As is, the conclusions about satellite data are just an assertion of the hypothesis of their applicability to plunge point monitoring. Furthermore, it seems clear that the other two conclusions, which refer to the dynamics of the reservoir, not to the plunge point, are not true.

More precisely, the second conclusion says (P1207L7-8): “For wet season the upward entrainment engendered by the Kelvin-Helmholtz instability was the main mixing mechanism in reservoir waters”. But in P1194L11.12 it is said that “the river flowing as underflow contributes to the thermal stability of the water column during the wet season” and in P1203L1-3 we read that an “analysis based on Wedderburn and Lake Number (not shown) indicated that the wind-driven upwelling/downwelling was unimportant during the wet season”. No definition or references for these numbers are given in the text . So, how can K-H dominate the mixing? It seems that the authors imagine the epilimnion moving due to the wind and the hypolimnion due to the underflow and that (P1205L8-9) “the Kelvin-Helmholtz may play a strong role in the thermocline displacement during the wet season”. Accordingly, they calculate a bulk Richardson number at the thermocline considering for the term (U1-U2) in the denominator the following (P1205L6-7): “estimates of U1 obtained from u* and U2 calculated from the inflow and measured by drifters”. In P1201L18-19 it is said “the friction velocity was calculated as where V is the surface wind speed". The only sentence in the text about the value of the velocity of the flow is (P1204L6-7): "If moving with a propagation speed u $\sim$ 0.1 ms-1 (as estimated by model and confirmed by drifters)". I suggest the authors read, for instance, Imberger, J. 1998, Flux paths in a stratified lake: A review in Physical Processes in Lakes and Oceans, American Geophysical Union , Washington, D.C. and rethink their analysis.

Regarding the third conclusion (P1207L9-11), it says nothing about any of the objectives of the paper.

The section on methods looks rather eclectic, referring to measurements not used in the text and including a subsection with a very simple discussion of the theoretical background of density currents and a lack of relevant references on the topic.

Apart from conceptual problems in the text, other errors or examples of looseness should be avoided. Some examples are:

P1202L23-25: "Due to the weaker winds, the atmosphere's reservoir boundary layer is less turbulent, advection and diffusive contributions to sensible heat flux and evaporation are all small and the radiational heat input is concentrated in a shallower surface layer (Fig. 3)". I understand that "raditional heat" refers to the radiative fluxes. In this case, it is true that long-wave radiation affects only a thin layer at the surface but short-wave radiation always penetrates into the water column, although it could be absorbed rather quickly depending on the water characteristics, not on the non-turbulent state of the atmospheric boundary layer. In any case, Figure 3 is a contour plot of temperature profiles recorded with a thermistor chain, so it provides no direct information about the transfer of short-wave radiation in the water column.

Figure 2.- Mean daily short-wave and long-wave data are presented but it will also be interesting to see the other components of the surface heat balance to support a convective regime as it is very inaccurately said in the text (P1202L14-19): "As latent and sensible flux terms are almost linearly dependent on wind and the short waves

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drops during the cold front passage, the net surface radiation becomes negative and reservoir loses (latent) heat to the atmosphere (not shown). Thus, the deepening of the surface mixing layer due to the stirring from the wind stress will be enhanced by downward convective mixing in dry seasons."

Many stations have been indicated in Figure 1 (in fact more than those which are used in the text) but in Figure 3 it is said that the thermistor chain was "near the dam". Also it is not said at which stations data shown in Figures 8, 10 and 12 were recorded. The transect shown in Figure 7 should also be located in Figure 1. MAN40 station referred to in Figure 8 is neither indicated in Figure 1 nor, I believe, in the text. For what and where (see Figure 8) is the pH used in the text?

Some of the data shown in Table 1 cannot be the same for every year but the year is not specified. Is it 2007?

All in all, I would suggest the authors take some time to define a clear objective and rethink their work, which has to be presented more accurately. I would also mention the English language, a handicap for non-native speakers, like myself, which I understand very well. However, looking at sentences like the one starting on line 20 on page 1996: "The satellite images ...."), I recommend the authors ask for the support of a professional editor or a colleague fluent in English.

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