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Interactive comment on “Effects of bottom topography on dynamics of river discharges in tidal regions: case study of twin plumes in Taiwan Strait” by K. A. Korotenko et al.

K. A. Korotenko et al.

kkorotenko@gmail.com

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Dear Professor A. Souza:

Thank you very much for your critical comments and suggestions. We do agree with most of them. Based on your comments, we introduced significant changes in the revised manuscript. These changes and our responses to the criticisms are listed below.

Comment. This paper shows the results of numerical simulations of the Taiwan Strait on an attempt to explain difference on the behaviour of two river plumes as they approach the coastal ocean. I suspect that the manuscript attempts to describe how

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differences on tidal mixing produce differences in the plume structure or behaviour. Nevertheless this is neither clearly stated nor proven. The manuscript is poorly written and badly organised, I cannot follow a coherent idea of what the authors are attempting to do I only see a series of model results with no clear train of thought. Response. We significantly re-arranged and extended the paper, complementing it with new simulations (new Figs. 8 and 9) intended to support the central statements of the article. We also tried to improve the logical structure of the text.

C. One of the main problems I have with the article is that, although the authors explain at great length an observation campaign, this data is never presented and never used for model validation and explanation of what processes are involved in this ROFI. R. The field data in our possession are indeed limited. However, we do use observational data to compare the measured velocity record and salinity distribution with the outcome from our simulations (Figs. 2 and 3). Also, in the revised article, we eliminated the section formerly called “Field campaign” merging it with the “Study region” section.

C. I would agree with reviewer on that the use of STRIPE is very questionable as the comparison of the wetting and drying would be done better in an Eulerian model, such as GETM, ROMS or POLCOMS R. Please see our reply to the same comment from J. Simpson.

C. The authors need to review the current literature including work carried by our group at NOC, as well as and work carried by Buchard, De Boer, Valle-Levinson, Monismith, Geyer and MaCready amongst others to carry out a better work on assessing the dominant processes controlling the different plumes. R. We agree with this comment. In response, we significantly extended our literature review and included the abovementioned references. We also included a brief description of the tidal straining processes together with the corresponding citations.

C. An immediate comment is to substitute the use of the Simpson and Hunter parameter which was devised for the heating and stirring case in which the tidal stirring

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represented as the dissipation as per your equation 12 and balances by the buoyancy input due to heating so that the proper nondimensional number would be BH/U_*^2 . With B as the surface heat buoyancy flux H water depth and U_* the frictional velocity. You could probably change the Buoyancy heat flux by the lateral freshwater buoyancy, but this is better explained using the horizontal Richardson number $Ri_H = g/\rho_0 \frac{d\rho_0/dx}{H_*^2/U_*^2}$ as mention in Monismith et al 1996. This should give you an idea of how the water column structure in the ROFI. R. Following your advice (thank you), we computed the horizontal Richardson number (new Fig. 8) which gave and an additional confirmation of dominant effect of tidal mixing above the Chang-Yuen Ridge on destruction of the plume. Additionally, we performed new computations to assess the contribution of buoyant production/destruction rate term in the equation of TKE balance and presented those results to show that the “competition” between the shear-production and buoyant destruction rates played a dominant role in the formation of those differences in behavior of the two plumes (new Fig. 9). As to the Simpson-Hunter parameter, we decided to retain it in the original form to compare with observational data by Zhu et al. (2013) for Taiwan Strait, as it seems quite illustrative.

C. I think that the manuscript cannot be published in Ocean Science until the scientific arguments are better prepared, the description of the study area is improved and the model used is properly validated. R. We tried our best to improve the manuscript and meet your suggestions.

On behalf of all authors, Konstantin Korotenko

Interactive comment on Ocean Sci. Discuss., 11, 1149, 2014.

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