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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.

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Dear Professor J. H. Simpson: 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 raises the potentially interesting question of how the structure of river plumes entering the coastal ocean are affected by the ambient tidal mixing regime. In my view, the results presented do not fully illuminate the contrast between the two plume regions, nor do they convincingly identify the processes involved. Response.

Preparing the revised article, we conducted new computations aimed at assessing the contribution of buoyant production/destruction rate term in the equation of the TKE budget, and included these results to the manuscript in order to demonstrate that the "competition" between shear-production and buoyant destruction rates played a dominant role in the formation of the differences in the behavior of the two plumes. To this end, we substituted Fig. 9 by a new one with a plot of two zonal sections of buoyant destruction rate for both river plumes during the tidal cycle. The sections revealed greater suppression of turbulent energy by buoyancy in the upper 15 m depth during formation/evolution of Wu River's plume than those occurred during development of the plume of Zhoushui River. We also added an evaluation of horizontal Richardson number. The latter helped to explain the considerable dissimilarity of stratification development in the area of Zhuoshui and Wu Rivers associated directly with tidal mixing above the CY ridge and away from it (Fig. 8).

C. In part this is because there is no adequate observational data base with which to challenge the model output R. Observational data that could validate our simulations are presented in Figs.2 and 3.

C. [. . .The authors] do demonstrate that the level of tidal stirring is different in the two cases, mainly because of the different topography, but they do not use the model to discriminate between the effects of vertical mixing (manifest in its effect on tidal shear diffusion?), horizontal mixing and WAD (wetting and drying) effects associated with coastline differences. R. We agree with this, at least partly, given that it was our initial intention to focus on the presumably dominant effect associated with the bottom topography differences. However, horizontal mixing (see discussion of Fig. 11) and WAD effects are also discussed in the paper. We distinguished the effects of the mixing from the WAD effects in the following manner. Initially we performed both POM and STRiPE simulations without WAD. After establishing good agreement between these two models in simulating the non-WAD scenario (i.e., horizontal and vertical mixing effects taken alone), we then performed STRiPE simulations supplemented by WAD forcing. Therefore, the results presented in Fig. 14 provide some insights into the relations between the mixing effects and WAD effects on the river plumes.

C. The use of a second model (STRIPE) seems questionable at least until it has been shown to produce results consistent with those from POM for non-WAD scenarios; wouldn't a model with a proven WAD capability (e.g. GETM) have been a better choice for all runs ? R. STRiPE is a newly developed model which has been specifically designed for simulating dynamics of small and medium size river plumes. It has demonstrated its ability to produce realistic results consistent with the in situ measurements and satellite observations of different river plumes. Also, STRiPE has been tested under idealized forcing conditions against the analytical solutions known from previous studies. Detailed discussion about the model validation as well as about advantages and disadvantages of its use is given in [Osadchiev and Zavialov, Continental Shelf Research, 2013, 58, 96-106]. We decided to use STRiPE to investigate the role of WAD effects on river plume behavior because of its low "computational cost" and relative easiness of simulating a variety of aspects of plume dynamics comparing to Eulerian models. However, we fully agree with you that STRiPE should be validated for the study region against a trustworthy and widely used Eulerian model. Therefore, in the revised paper, we added a comparison between POM and STRiPE outcomes. To this end, the STRiPE and POM simulations were used for non-WAD settings of Zhuoshui and Wu river plumes and indeed showed a good level of agreement between each other. The newly added Figure 13 illustrates STRiPE and POM plume contours during the four successive flood and ebb phases.

C. In devising a revised strategy for model runs to clarify the mechanisms responsible for the differences between the two plumes, I would suggest that the authors consult more fully the now extensive literature on ROFI dynamics which describes, on the basis of detailed observations and modelling, the sometimes subtle processes operating in ROFIs (e.g. see the work of Gerben de Boer and co-authors as well as the modeling studies of Burchard and others). The presentation of the literature in the present draft amounts to little more than a list and does not do justice to the current state of process understanding in ROFIs.

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. The authors should also present a fuller picture of other differences between the two plumes, notably in spatial sections of density stratification, a feature of central importance in understanding processes which, though implicit in figure 10, is not shown. R. We presented new simulations and Fig. 9 indicating differences between the two plumes associated with time-depth variations of buoyancy destruction/production rate of TKE.

C. More observational data on such aspects would be helpful though I realize that such data may not be available without further observational campaigns. R. Unfortunately we are not able to extend our observational campaign. Instead, we tried to make use of the limited available data as a mean to validate the model (Fig. 3). Also, in the revised article, we eliminated the section formerly called “Field campaign” merging it with the “Study region” section.

C. There are many smaller points which need attention, particularly in the figures (e.g. figure 6 where the vectors cannot be resolved even after full zooming!). R. We prepared a high-resolution version of Figure 6 and will upload it on the OS website if the paper is accepted for publication. Some other figures were also redrawn with higher quality.

C. As I see it, however the paper needs an extensive scientific upgrade before it would qualify for publication in OS and the priority now is to address the fundamental issues indicated above. R. We tried our best to upgrade the manuscript.

On behalf of all authors, Konstantin Korotenko

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