Ocean Sci. Discuss.,
doi:10.5194/os-2017-1-AC1, 2017

Ocean Science Discussions

Interactive comment on "Small river plumes off the north-eastern coast of the Black Sea under average climatic and flooding discharge conditions" *by* Alexander Osadchiev and Evgeniya Korshenko

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Received and published: 4 May 2017

C: While the focus is on salinity and suspended sediments, what are the consequences for the nutrients coming down the rivers? Global, catchment-model-based estimates of annual river discharges and nutrient loads (e.g. the NEWS 2 database, see Mayorga et al., Env. Modelling Software, 2010) which, for the majority of rivers around the world, represent the best information currently available. These models tend to provide discharges and loads in an annual mean sense – so what are the implications of this new work, for both suspended material and also for nutrients? It struck me that one big

difference between the few large rivers and the many smaller ones would be river length and, possibly, the catchment type being drained. It would be worth some sensible speculation in the discussion to consider the implications of the work for the use of such global databases.

R: Thank you for these valuable suggestions. Spatial resolutions of basins and river networks used in these models are not enough to reproduce small rivers as those addressed in this work. As a result, these models neglect the role of small rivers in nutrient and sediment loads. However, the extreme flooding events can, first, significantly influence freshwater, nutrient, and sediment land-ocean fluxes at least at a synoptic time scale, and, second, have significantly different patterns of coastal transport of dissolved and suspended water constituents as it was shown in this work for RCBS. Thus, this study can be used for estimation of the role of small rivers in nutrient and sediment loads at certain world coastal areas with similar river discharge configurations which are neglected by the global catchment-based models. The related discussion was added to the manuscript.

C: There is some discussion (page 34, lines 9-13 or so) on trends in event-driven discharges. Are these climate-change driven or local regional natural variability? Either way, it could be clarified, but also the possible climate-driven changes to more extreme events generally could be drawn out more here.

R: Thank you for this comment. These trends are climate-change driven which was clarified in the manuscript. Also according to your suggestion we added a discussion about the climate-driven changes in frequency and duration of the intense and extreme precipitation events in the study region.

C: The validation of the satellite-derived suspended material data with river discharge could be more robustly demonstrated, e.g. plots of ln(C) vs ln(Q) for large and small rivers, and a demonstration that the correlation coefficients are significant.

R: According to your recommendations we added the plots of ln(C) and ln(Q) for the

large and small rivers of the study region (Fig. 5) and the relevant discussion to Section 4 to demonstrate that the correlation coefficients are significant.

C: Some of the details in the model configuration could perhaps be edited out, as there are sufficient published studies that have already set up the model that can provide these. Section 6.1 seemed a bit out of place. Either the validation of the model should be part of the methods, or it should at least occur at the start of the results section.

R: We agree that some details of the model configuration given in Section 5 can be edited out, especially concerning the STRiPE and sediment transport models. According to your suggestions we significantly shortened the model description in Section 5 and moved the model validation from Section 6.1 to Section 5.

C: The continuous along-shore low salinity plume (line 10, page 18) is noted as being 5 – 15 km in width. How does this compare to the local internal Rossby radius?

R: Many thanks for this comment. The cross-shore extent if this strip was $\sim$5 km, however, in proximity of the estuaries of the large rivers of RCBS it increased up to 10-15 km. This is consistent with the value of the local internal Rossby radius equal to 5 km. This clarification was added to the text.

C: Page 21, lines 11-12. Check the sediment load numbers – they are different by 2 orders of magnitude, and they don't seem consistent with the statement immediately following about the "real" system being 25% greater.

R: Thank you for this point, corrected.

C: There is heavy use of abbreviations in the manuscript (e.g. RCBS, GCR) which gets confusing at times. My preference is to avoid abbreviations unless they are very widely accepted – the text flows better without the reader having to keep reminding themselves what an abbreviation stands for.

R: We agree that the multiple abbreviations used in the manuscript are confusing and can be mostly avoided. We replaced all abbreviations except RCBS (Russian coast of

the Black Sea), which is the longest and the mostly often used (58 times) and several well-known and widely used like TSM, CDOM, SST, SSS, WRF, STRiPE, INMOM.

C: The manuscript will need some careful checking for editing/clarification of English – though it is generally very well written.

R: Thank you for this comment. The revised manuscript was proofread by an expert English speaker, which significantly improved its language and style.

On behalf of all authors, Alexander Osadchiev

Please also note the supplement to this comment:
http://www.ocean-sci-discuss.net/os-2017-1/os-2017-1-AC1-supplement.pdf

Interactive comment on Ocean Sci. Discuss., doi:10.5194/os-2017-1, 2017.