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Interactive Comment

Interactive comment on "River bulge evolution and dynamics in a non-tidal sea – Daugava River plume in the Gulf of Riga, Baltic Sea" *by* E. Soosaar et al.

E. Soosaar et al.

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Comment 1. The paper describes many details of the observation and simulation that without enough explanation of why these features occur. It should be more concise and deliver a more focused "story" the readers.

Reply: We have tried to rewrite the paper in a more concise manner and to give more explanations.

Comment 2. It is mentioned in the paper that the tidal oscillation is small in the study region, thus tide forcing is not considered, however an additional case with tide forcing should be considered to approve that the tidal influence on the river bulge is negligible, as many studies show that tide-induced mixing have significant effect on the structure of the river plume (Chao, 1990; MacCready et al., 2009; Zu et al., 2014)

Reply: There is a big difference in the magnitude of tidal oscillation between noted studies and tides in the Baltic Sea and the GoR. MacCready et al., (2009) study is based on Columbia River and Zu et al. (2014) is looking at a Pearl River Estuary in South China Sea where tidal oscillations are in the range of 2-6m). In the Baltic Sea, tides vary in range 1-10 cm, which is mentioned in the text. In comparison with other forcing acting on the river bulge (wind, variation in river flow), tides in the GoR are marginal. There are no studies on the effect of tidal mixing to the overall vertical mixing in the Baltic Sea. Lilover has calculated the contribution of the tidal shear on the gradient Richardson number in the Gulf of Finland. Results show that tidal contribution is about 10-20% if Ri = 1 (personal communication). Our ten month long measurements of currents in the GoR with ADCP show that tidal constituents are negligible in the current velocity spectrum (unpublished).

Comment 3. By using the term balance on a certain time, the paper concludes that geostrophic balance is valid for the entire mid-field of the bulge, which is different with previous results. However, time series of the term balance should be presented to approve that the conclusion is universal here, and is not an occasional event.

Reply: We calculated the time series of spatially averaged momentum balance terms, Eq. (3), for an ideal and real bulge and added a paragraph and Fig. 9 to the revised manuscript.

Comment 4. As many results are based on the model simulation, more detailed description of the model set up is needed, (i.e. numerical scheme, physical forcing, open boundary conditions ...)

Reply: Additional information was added to the model description section in the revised manuscript.

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Comment 5.1 On page 4, at line 20-28, the author should add some short statement about the circulation in the Baltic Sea.

Reply: We assume that the reviewer meant the Gulf of Riga. As the GoR is a nearly closed gulf that connects with the Baltic Sea through two narrow straights, circulation in the Baltic Sea does not have relevant effect on the circulation in the GoR. We added a paragraph in the revised ms.

Comment 5.2 The observed representative T/S profile should be plotted in Figure 1. Reply: T/S profiles were added to the Figure 1 in the revised ms.

Comment 6. On page 6, at line 20-21, the Baltic Sea has two large straits (Irbe Strait and Virtsu Strait) and these strait has important effects on the circulation in the Baltic Sea, why the model use the closed boundaries?

Reply: We assume that the reviewer meant the Gulf of Riga. In the study by Soosaar et al. (2014) it is quantified that in case of over a month long simulation correlation between circulation created in a closed and open boundary, simulation has R2 = 0.93. Differences were located in the immediate proximity of the straits. Our simulation concentrated on the south-east part of the GoR where the influence is negligible over a time period of two weeks, i.e. the model simulation period. An explanatory text is added to the revised ms.

Comment 7. On page 7, at line 5-6, the spin-up time of the model only 3 days. It seems too short?

Reply: A 3-day model spin-up time was used to smooth out small-scale spatial salinity discontinuities due to interpolation and to allow smooth input of fresh water from rivers to ambient saline water. As initial salinity fields were interpolated from the 1 nautical mile simulation for the Baltic Sea (Maljutenko and Raudsepp 2014) on 20 March 2007, we expect that the density field is consistent with hydrodynamical and meteorological conditions on 20 March 2007. Therefore, we expect that a 3-day geostrophic adjust-

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ment is sufficient to spin-up the velocity field. Using longer spin-up without wind forcing would result in considerably different salinity/density fields that were characteristic for 20 March 2007.

Comment 8. On page 12, at line 5, "the pulsation of the actual bulge" should be "the pulsation of the real bulge".

Reply: Corrected.

Comment 9. On page 12, at line 5-10 (shown in Figure 5). For ideal bulge, when bulge diameter increased, bulge mean depth increased. The statement “when bulge diameter increased, bulge mean depth decreased and vice versa.” why?

Reply: We suggest that the fresh water volume in the bulge is roughly conserved. Therefore, the bulge depth responds to lateral bulge fluctuations. As the bulge area extends, the bulge depth decreases and vice versa. We do not have detailed explanation and future study is needed to have a solid answer, which we think is beyond the aim of this paper. Therefore, we like to keep it as a notification without any speculation provided in the text.

Comment 10. On page 14, at line 5-15, The real simulation gave rb$\sim$t0.50$\pm$0.04 while the ideal simulation gave rb$\sim$t0.28$\pm$0.0. And the satellite remote sensing gave rb$\sim$t0.31$\pm$0.23. Why is the evolution of the bulge in the ideal simulation more similar to that in the satellite remote sensing?

Reply: The approximation of the growth rate of the bulge radius from satellite remote sensing has large uncertainty (0.23) as there are too few satellite images available. We have removed from the revised manuscript the parts where bulge measures from satellite images are defined, described, calculated and discussed. Thus, the approximation, rb$\sim$t0.31$\pm$0.23, was removed from the revised manuscript.

Comment 11. On page 15, at line 24, “ one km” should be “1km”

Reply: Corrected.

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Comment 12. On page 16, at line 24, the bulge centre should be defined using geometric mean position of the distribution of the tracer concentration. The reason is that when ambient current overrode bulge circulation, the bulge centre was not defined using with closed streamlines, although the bulge still existed if we look at the distribution of the tracer concentration.

Reply: The bulge centre is defined from the velocity field because we calculate momentum balance and movement of the bulge centre. Momentum balance is calculated in a cylindrical coordinate system where the origin of coordinates is located at the point where angular and radial velocity components are zero. This definition is consistent with previous studies that address momentum balance calculation inside the bulge and movement of the bulge centre (Nof and Pichevin 2001; Horner-Devine et al. 2006; Horner-Devine 2009).

We have calculated the bulge centre as a geostrophic mean position of the distribution of the tracer concentration and from the velocity field for the real bulge (enclosed with answers to the reviewer, Fig. 1). In the case of geometric mean, the bulge centre is close to the river mouth, but 5 km from the bulge centre defined from the velocity field. Placing the origin of cylindrical coordinates in the geometric mean positions and calculating the momentum balance hampers the interpretation and comparison of our results with previous studies. Over the course of the model simulations, the centres of ideal and real bulge defined using geometric mean remain closer to the river mouth than the centres from the velocity field (not shown).

Taking into account the above argumentation, we did not make changes in the revised manuscript.

Comment 13. On page 18, at line 20-22, why the bulge centre was closer to the coast in the case of the ideal bulge than in the case of the real bulge?

Reply: We rephrased this sentence, so that in now offers an explanation there.

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Comment 14. On page 30,what is the meaning of white blank area in the bulge in Figure 6 (left column)?

Reply: The blank area within the bulge is where the tracer concentrations were below the threshold values of the bulge definition. This is explained in the figure caption. We added (see text for bulge definition) in the figure caption of the revised ms.

Reference

Chao, S.-Y., 1990. Tidal modulation of estuarine plumes. J. Phys. Oceanogr. 20, 1115–1123.

MacCready, P., Banas, N.S., Hickey, B.M., Dever, E.P., Liu, Y., 2009.Amodel study of tide- and wind-induced mixing in the Columbia River estuary and plume. Cont. Shelf Res. 29,278–291.

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Maljutenko, I. and Raudsepp, U.: Validation of GETM model simulated long-term salinity fields in the pathway of saltwater transport in response to the Major Baltic Inflows in the Baltic Sea. IEEE/OES Baltic International Symposium (BALTIC), 2014.

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Pan, J., Gu, Y., and Wang, D.: Observations and numerical modeling of the Pearl River plume in summer season. J. Geophys. Res.: Oceans, 119(4), 2480-2500, 2014.

Horner-Devine, A.R, Fong, D. A., Monismith, S. G. and Maxworthy, T.. Laboratory experiments simulating a coastal river inflow. J. Fluid Mech. 555, 203-232, 2006.

Horner-Devine, A.R.: The bulge circulation in the Columbia River plume., Cont. Shelf Res., 29, 234-251, 2009.

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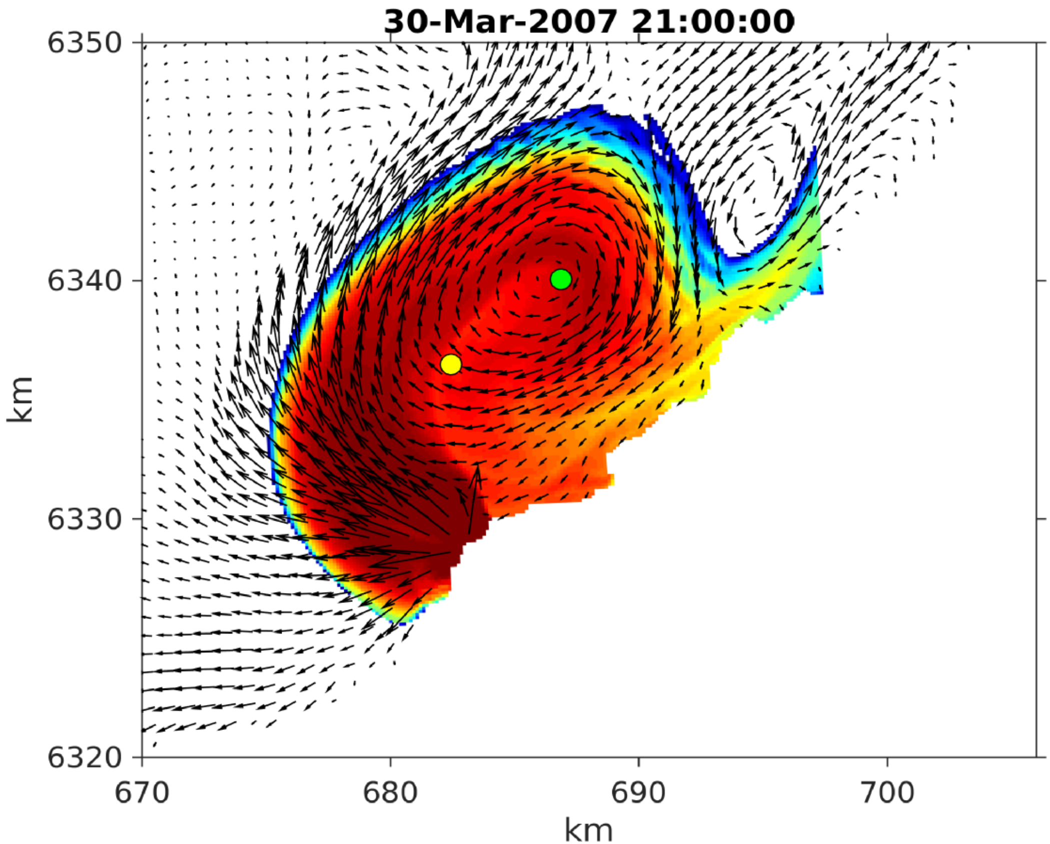

Fig. 1. The bulge centre defined from the velocity (green dot) and as geometric mean position of the distribution of the tracer concentration field (yellow dot)

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