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Interactive comment on "Water exchange between the Sea of Azov and the Black Sea through the Kerch Strait" *by* Ivan Zavialov and Alexander Osadchiev

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In this paper, the authors consider the Sea of Azov (SA) as an estuary and investigate a flow exchange between SA and the adjacent Black Sea (BS) by means of remote sensing. As a proxy for the buoyant outflow from SA into BS they use Cl-a, although on page 5 (lines 22-25) they state that SST, Cl-a and TSM "are prone to significant variability and/or do not act as passive tracers, which hinders their direct application for accurate identification" of buoyant outflow from SA. As a proxy for the BS water inflow into SA, the authors use TSM, which they have discarded as a reliable marker for buoyant water just one page earlier (i.e., page 7, lines 5-8). They have to rely on TSM signal now, because the BC inflow propagates near the bottom and does not produce immediate signature on the surface. The major conclusion of this study is that the buoyant outflow from SA into BS occurs only under the external forcing by northeasterly (NE) winds, while inflow of heavy BS water into SA occurs during the relaxation of NE winds (e.g., page 10, line 24) and under any wind conditions. The authors also attempt to quantify "intensity" of the flow exchange through the Kerch Strait by determining the area of the SA plume in BS, and scaling this area against the integral of wind speed when the wind is favorable for the plume formation. In my opinion, there is a major confusion in this conclusion: do the authors imply that NE winds precondition the BS inflow into SA (that is, relaxation of NE winds triggers the BS inflow) or do they mean that the BS inflow occurs always as long as NE winds do not operate? The authors seem to interchange these two very different messages in different parts of their manuscript (e.g., compare lines 24-26 on page 10 and lines 23-26 on page 11). I feel strongly that the estuarine exchange flow should occur in both directions, but the authors seem to imply that under light or no wind forcing conditions, it is a one-way traffic: BS water flows in SA in a unidirectional manner. This is a bold and unsubstantiated claim. Frankly, I don't think that TSM signal can provide any reliable information about the presence of BS water at the bottom, it just tells us about the wind-induced resuspension of sediments. Apart from this major issue with paper's conclusions, I have some questions with the analysis. I am not sure why the authors use logarithms of properties compared in Figure 6 (and not the properties themselves). The bottom panel scatterplot does not resemble a linear function, so the linear regression is a poor choice here. This relationship is by no means linear: first, the wind driven transport is proportional to the wind stress (which is quadratic in wind speed). Second, the surface area is not directly proportional to the discharge Q through the Kerch Strait, but rather to Q/h, where h is the sickness of the plume, which is generally unknown but does depend on the wind stress. Regarding the upper panel of Figure 6, I think the alongshore extension of the plume under the downwelling wind forcing primarily depends on the ALONG-SHELF wind stress component (not the wind velocity magnitude). Throughout the paper, there is unfortunate confusion about the wind direction. In geosciences, wind direction indicates where the wind blows from and it is measured from true north in clockwise direction. Wind direction 210 to 260 degrees (page 6, line 17) means that the wind blows from southwest, and it's not what the authors assume. Likewise, "northeastern" (wind) and "northeastward" have opposite meanings, northeastward denotes a flow from southwest towards northeast, but the authors freely interchange these two terms (e.g., lines 24 and 26 on page 10, and many other instances). On page 11 (2nd paragraph), the authors compare the SA volume and the annual volume of the freshwater discharge. This is a very strange proposition because a year is not a proper time scale for the exchange processes between AS and BC, according to the authors. In my opinion, the authors should rectify their arguments in this part of the discussion. Finally, the language should be checked throughout. For instance, it is a "boundary condition", not a "border condition". Word "significant" is badly overused. In scientific literature, this word tends to have a statistical context (that is, significant in quantitative, statistical sense). "Substantial" would probably sound better and less irritating for quantitatively minded readers. In conclusion, I recommend this manuscript to be returned to the authors for major revisions. I think the analysis of Cl-a can be salvaged, but it is unclear at this point if it will account for the full publishable unit.

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