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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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We strongly appreciate the reviewers' suggestions and comments that served to improve the article. In response to them, first, we added results of numerical modelling that supported the results obtained from analysis of satellite imagery. Second, we clarified methods of detection of surface-advected outflows from the Sea of Azov to the Black Sea and bottom-advected inflows in the opposite direction based on agreement between Chl-a and TSM satellite imagery. Third, we modified the interpretation of influence of wing forcing on inflows from the Black Sea to the Sea of Azov and provided a more thorough physical background based on both satellite data and numerical mod-

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Discussion paper

elling. Finally, we adopted many specific comments and recommendations from the reviewers, reworked the structure of the manuscript, and improved the quality of English text and figures. The detailed reply to reviewers comments and description of changes made in the manuscript are given below and in the attached file.

1. 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.

Response:

Thank you for this valuable comment. SST, Chl-a, and TSM indeed are prone to significant variability and straightforward usage of these individual characteristics for detection of outflow from SA to BS can be misleading. Thus, in the revised version of the manuscript we used more comprehensive joint analysis of SST, Chl-a, and TSM. Inflow events from SA to BS were identified by elevated concentration of Chl-a in the Kerch Strait and the adjacent coastal area of the Black Sea, because Chl-a has the lowest short-term variability among the considered sea surface characteristics and are not affected by wind/wave-induced sediment resuspension. If an inflow event was detected, we analyzed areas of elevated TSM, Chl-a, and elevated (in summer) or reduced (in winter) SST associated with formation of the Azov plume (AP) in the northeastern part of the Black Sea. If general forms and spatial scales of these areas were similar, we defined borders of AP basing on gradient of TSM that is the most stable passive tracer of AP in absence of episodic wind-induced bottom resuspension events. If areas of TSM, Chl-a, and SST anomalies were not consistent with each other, we assumed that areas of elevated TSM and reduced SST (in winter) were modified by wind-induced resuspension and mixing of AP with subjacent sea. In this case we defined borders of AP basing on gradient of Chl-a that is the most stable tracer of AP during intense wind forcing conditions. These issues were described in the text. Also we applied a calibrated hydrodynamic modelling using the BSAS12 model to prove the results obtained from the analysis of satellite imagery. Salinity distributions simulated by BSAS12 in the study region showed good agreement with detection of AP and BP at satellite imagery, that is explicitly described in the revised version of the manuscript and illustrated by Figures 2, 8, 11. Thus, the applied method of identification of AP and BP and the obtained results are supported by both satellite data and numerical modelling.

2. 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 BS inflow propagates near the bottom and does not produce immediate signature on the surface. 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.

Response:

TSM is not a reliable marker of SA water in the Black Sea because TSM increases over shallow areas in case of resuspension of bottom sediments. Thus, areas of elevated TSM can be induced by both sediment resuspension and spreading of SA water in the Black Sea. However, spreading of BS water in the Sea of Azov is indicated by reduced values of TSM in the shallow area adjacent to the Kerch Strait. Thus, resuspension of bottom sediments which increases TSM at the study region can not be misleading for detection of SA water indicated by reduced TSM especially at shallow areas. Thus, TSM is not a reliable marker of SA water in the Black Sea, but is a reliable marker of BS water in the Sea of Azov. Moreover, in the revised version we analyzed both TSM and Chl-a, and identified SA water in the Black Sea as areas of reduced TSM and Chl-a. This procedure provides more confidence in detection of SA water, as compared to usage of only TSM. As a result, the scheme of identification of inflow events from the Black Sea to the Sea of Azov and detection of borders of the bottom-advected Black Sea plume (BP) at satellite imagery is relatively straightforward, as compared to identification of inflow events from the Sea of Azov to the Black Sea. However, we assume that BP is not manifested by anomalies of TSM and Chl-a in the surface layer during low wind forcing conditions, while during strong wind forcing conditions resuspension of bottom sediments can induce elevated concentrations of TSM in the surface layer that also hinders identification of BP. As a result, many of BI events are not detected by optical satellite imagery.

3. 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 relaxation of NE winds 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).

Response:

Many thanks for this important comment. In the revised version of the manuscript we clarified meaning of "relaxation of NE winds" related to inflow of BS waters into SA. We state that inflow of BS waters into SA occurs after reverse of a barotropic pressure gradient as a result of relaxation of NE winds, i.e. relaxation of NE winds triggers the BS inflow. This result was supported by numerical modelling.

4. 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.

Response:

Many thanks for this important comment. We totally agree that water exchange through the mouth of a positive estuary is typically a two-way process with in- and outflows happening at the same time. However, it is not the case of narrow and shallow central part of the Kerch Strait, which width and depth are equal to 3 km and 3-4 m. In absence of external forcing water exchange through the Kerch Strait indeed is a two-way process. However, due to the dominating role of a barotropic component in the total pressure gradient along the strait (Figure 5 in the revised version of the manuscript), even moderate wind forcing at the study region induces a one-way water exchange in the narrowest part of the strait which defines water exchange between the Azov and Black seas. Numerical simulations show that a two-way water exchange occurred in the Kerch Strait only during weak wind forcing conditions which total annual duration was 34 – 54 days in 1992 – 2010, which is only 9-15% of the whole year (Table 1). Thus, one-way water exchange was observed during the majority of the year. This result is also supported by previous in situ observations (Ivanov, 2011) and numerical modelling (Stanev et al., 2017) of water exchange in the Kerch Strait. The rest of the year a one-way water exchange was observed. This point was described in the text.

5. 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.

Response:

We agree that scatterplots in this figure (Figure 9 in the revised version of the manuscript) do not resemble linear functions, so the linear regression is a poor choice. In the revised version of the manuscript we improved these dependences and their approximations. Alongshore extent and area of AP increase with increase of wind forcing index. However, this increase is not steady, its derivative decreases with increase of wind forcing index. In particular if wind forcing index exceeds 3500 km, alongshore extent and area of AP are almost stable. Thus, the observed forms of dependences between have good approximation by logarithmic functions. We are not aware that logarithmic approximation has any straightforward physical background (despite steady decrease of dependence derivative) due to complex and strongly non-linear dependences between wind speed, discharge through the Kerch Strait, and spatial characteristics of AP. However, the obtained relations are essential for numerical parameterizations of water exchange through the Kerch Strait based on wind data, which are addressed in this study.

6. 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).

Response:

We agree with this point. Spatial scales of a buoyant plume in a non-tidal sea are defined mainly by two external forcing parameters, namely, river discharge rate and wind forcing. Thus, alongshore extent of AP depends on discharge rate of SA water through the Kerch Strait and local wind forcing. Inflow of water from SA to BS and formation of AP occurs only under north-easterly wind forcing. As a result, cross-shelf wind component determines discharge rate of SA water through the Kerch Strait and both wind components govern further alongshore extension of AP. In this study, we consider alongshore extension and area of AP only during periods of inflow from SA to

BS, i.e., we do not consider AP detached from its source in the Kerch Strait after reverse of flow direction in the strait and cessation of inflow from SA to BS. Thus, we calculate alongshore extension and area of AP only during moderate and strong north-easterly winds that induce inflow from SA to BS. During these periods, alongshore extension of AP indeed shows good relation with variability both cross- shelf and along-shelf wind component. However, the dependence of alongshore extension of AP on wind velocity magnitude is better due to small range of the considered wind directions. This issue was clarified in the text.

7. 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). Response: Thank you for this point, we checked all parts of the manuscript where wind direction was mentioned and corrected the wrong usage of the related words.

8. 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 BS, according to the authors. In my opinion, the authors should rectify their arguments in this part of the discussion.

Response:

We agree with the reviewer that we should improve arguments in this part of the discussion. In this paragraph we discuss relation between freshwater discharge to river estuary and water exchange between the estuary and open sea, i.e. does signal of river discharge dissipate in the estuary or it influences freshwater outflow from estuary to open sea. This process depends on two main factors, namely, volume of inflowing river discharge and spatial scales of an estuary. If large river inflows to small estuary (e.g., the Amur River and the Amur Liman), variability of river discharge rate strongly affects water outflow from estuary to open sea. On the other hand, discharge of a small river inflowing to a large estuary does not influence and water exchange between the estuary and open sea. The area and volume of the Sea of Azov are large enough; therefore, signals of synoptic and even significant seasonal variability of discharge of the DON and Kuban rivers dissipate in the Sea of Azov and do not influence formation and/or intensity of AI events. It was clarified in the manuscript

9. 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.

Response:

Thank you for these points, we adopted them.

Please also note the supplement to this comment:
https://www.ocean-sci-discuss.net/os-2019-2/os-2019-2-AC2-supplement.pdf

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