Review of Kirillin et al. (2025) - Consequences of the Aral Sea restoration for its present physical state: temperature, mixing, and oxygen regime

This study investigates the current mixing dynamics of the western basin of the North Aral Sea (Shevchenko Bay), 15 years after a dam was built as an attempt to save the North Aral Sea from desiccation. The study demonstrates from year-round measurements of thermal stratification and oxygen that a thin and low oxygen bottom stratified layer persists in summer as in dimictic lakes. However, inertial internal waves move the stratified layer around, causing the sloping sides of the Shevchenko Bay to be fully mixed several times a year as in polymictic lakes. The authors call this mixing regime "transitional" between dimictic and polymictic and show with 1D numerical simulations that an increase (decrease) in water level and light attenuation would bring the lake to a steady dimictic (polymictic) regime.

Although the physical processes quantified by the study are well known, the fact that they are observed in a unique, evolving system makes the findings of the study very interesting and suitable for publication in HESS. The analysis that combines year-round temperature and oxygen measurements, CTD profiles, satellite images and 1D numerical modelling is comprehensive and robust. Yet, several statements are misleading or lack detailed explanation. Most of my comments aim at improving the clarity and readability of the manuscript.

Specific comments :

1. The location of the measurements is different from typical monitoring at the deepest point of the lake, which should be more emphasized through the manuscript. In fact, the data has been collected on the **<u>sloping sides</u>** of the **<u>western basin</u>** of the North Aral Sea (Shevchenko Bay; Fig. 1), which (i) might not be representative of the other basins of the North Aral Sea (including the deeper, central basin) and (ii) differs from measurements at the deepest point of the Shevchenko Bay. The authors should clearly state in the abstract, results, and discussion that their analysis focuses exclusively on the western lake basin. Additionally, they should elaborate on any potential differences or similarities in mixing dynamics between the western and central basins. Knowing the presence of an east-west salinity gradient (l. 241) and a ~7 m deep sill between the two basins (Fig. 1), do the authors expect wind-driven or density-driven basin exchange that could have affected their measurements? The mooring misses the bottom 25% of the water column because it was not deployed at the deepest point of the Shevchenko Bay. An additional mooring at the deepest point would have provided essential information on the full water column, including the persistence of the bottom stratified layer in summer. The authors should clearly indicate this limitation in the methods and that the frequent mixing events (polymictic-type) were only observed on the sloping sides of the basin.

2. It would help the reader to better explain the past, current and future mixing regimes of the Aral Sea. First, explicitly define "dimictic" and "polymictic" where the mixing regime is introduced in l. 61-67 of the introduction and remove the word "dimictic" in l. 43. The comparison between the natural (dimictic), pre-restoration (polymictic?) and current (transitional) mixing regimes of the Aral Sea should also be clearer by (i) providing information in the abstract about the past mixing regime (l. 18) <u>before</u> describing the current mixing regime in l. 10 and (ii) using the natural mixing regime as a reference point through the manuscript. The end of the abstract and the conclusion do not provide a clear answer regarding the preferred regime: is the goal to return to the natural, dimictic regime?
The current, "intermediate" (l. 10) or "transitional" (l. 409, 496) mixing regime should be better explained as it could be incorrectly interpreted as a regime alternating between polymictic and dimictic temporally (i.e., between years), whereas this variation is spatial

(i.e., frequent mixing on the sloping sides, persistent stratified layer at the deepest point). As explained in my first comment, the authors should provide hypotheses about the persistence of summer stratification at the deepest point. They could estimate the percentage of the Shevchenko Bay that remains stratified (based on a 2-3 m stratified layer, l. 474) instead of using the term "small" (l. 11, 475, 486).

Regarding the future mixing regime, I suggest to explicitly mention the effects of an increase (or decrease) in water level and light attenuation instead of referring to "changes" or "shifts" which remain vague (for example in l. 20 and l. 96).

3. The information about the morphology of the Ara Sea and its history should be re-organized in a more logical way. Volume and surface areas given in the abstract (l. 2-3) and in the conclusion (l. 526-527) should be moved to the introduction where the Aral Sea is presented (e.g., l. 50-54). A few characteristics of the restoration project (e.g., construction of a dam) should be given as soon as the "Aral Sea restoration project" is mentioned in l. 37. The name of the river inflows and the dam must be given as soon as they are introduced in l. 40 and l. 48, respectively. The paragraph about the climate of the Aral Sea region (l. 84-88) should be moved earlier in the introduction, maybe just after the mixing regime has been introduced (l. 67). The importance of the North Aral Sea (l. 88-92) should also be stated earlier, where it is introduced in l. 40 for example. Differences between the Southern (hypersaline) and Northern (brackish) Aral Sea could also be developed in the introduction so that the comparison between the mixing dynamics of the Northern and Southern Aral Sea (l. 428-435) would be clearer. Is $T_{md} > T_f$ in the Southern Aral Sea?

4. The assumption that salinity has negligible effects on stratification (l. 132-133) must be more justified and nuanced. Could the authors compare estimates of vertical bottom density gradient from conductivity profiles and from temperature profiles (e.g., June CTD profile) to support their assumption? Although salinity-driven stratification is negligible in summer, salinity effects might still play a role during turnover periods, when water temperature reaches the temperature of maximum density. It seems that an inverse thermal stratification occurred in mid-November (Figs. 3a, 4), which would indicate a role of salinity in maintaining a (weak) stratification. Salt exclusion could also generate convection (ice growth) or stratification (ice melt) in winter. The model does not include this process (l. 201-203), but l. 448-454 state that salt exclusion is important in brackish lakes, which seems inconsistent.

   In addition, the model assumes constant salinity over time (l. 203), which differs from the continuous decrease in salinity observed after the dam construction (Fig. 2) and from the natural seasonal variability of salinity (l. 76-77). The authors should mention this model limitation and discuss the potential effects of a further decrease in salinity on the mixing dynamics: would a decrease in salinity reduce the thermal expansivity of water (Eq. 10) and the tendency of the lake to stratify?

5. The methods section lacks the following information for better clarity:
   - Maximal and mean depth of the North Aral Sea and the Shevchenko Bay.
   - Value of the gravity acceleration in l. 127 (instead of l. 216).
   - Formula of g' and h_{eq} in l. 142-143.
   - Name of the two sampling locations instead of dates (Fig. 1) and replace "the sampling site" accordingly in l. 118.
   - Meaning of the Burger number (l. 147) and application to rotational internal waves.
   - Formula for M and V in l. 168.
   - Conversion from conductivity to salinity, as it is used in l. 248-249.

- Calculation of the salinity-dependent density in the model (l. 209-224). The salinity effects are included in Eq. (9), Eq. (10) and in l. 224, but it is unclear how the density is calculated from those quantities. Is the density calculated with TEOS-10 in the model as for field data (l. 130)?
- In-situ measurement of Secchi depth (value given in l. 228).
- Varying parameters between the simulations (sensitivity analysis), instead of fixed parameters in l. 227. The authors should also explain why the selected lake depth of 7 m (mean depth of the entire North Aral Sea, l. 392) is smaller than the depth of 11 m given in l. 227, which I assume is the mean lake depth of the Shevchenko Bay.
- Remote sensing data and determination of ice-on and ice-off dates. It is currently unclear in l. 271 how these dates were defined, but I assume that they come from the satellite images (Fig. 4)? The reference to satellite images is also missing in the description of the spatial variability of the ice cover in l. 276-281.
- Direction of the z-axis. It seems downward and positive in Eq. (1), Fig. 3b and l. 331, but upward and negative in Eqs. (2) and (7).

6. The figures are visually clear, but they lack some information, especially in the captions and in their interpretation.

Figure 2:

- Are the conductivity profiles corrected for temperature?
- A short description of the oxygen profiles should be added to the text.
- Why is oxygen decreasing near the surface in 2016?
- Why is there a bottom increase in Chl-a in 2016 and 2018?

Figure 3:

- The temperature color bar orientation in (a) is confusing because it is merged with the x-axis. I suggest orienting it vertically on the right of the plot.
- Are the transitions between lake seasons defined visually or based on N^2 or on the heat fluxes?
- How was the stratification threshold $N^2 > 2 \times 10^{-3}$ s^{-2} selected and what does the thick black isoline provide?
- Indicate years of measurements in the caption.
- Would it be possible to add wind speed data from a meteo station or from ERA5 to explain internal-waves generation (or in Fig. 5) and to support the wind stress value given in l. 340?
- Could the ice cover period be indicated based on satellite images as in Fig. 4?
- Use lowercase letter for subpanels as in the other figures.

Figure 4: mention in the caption that the dot on satellite images shows the mooring location.

Figure 5:

- The frequency σ does not match the dimensionless frequency defined in l. 158: I assume that it should be ω instead? Same in Fig. 8.
- Explicitly indicate that 4.5 d is a period and not a frequency, by moving the text away from the x-axis or relating it to the frequency as $\frac{2\pi}{\omega} = 4.5$ d.
- Use a different line style to indicate the Coriolis parameter f as it can be confused with the $S(\omega)$ relationships, and mention this line in the caption.

- Indicate the meaning of the horizontal and vertical grey solid lines (e.g., value of the Burger number $S = 0.12$).

Figure 7:
- DO units are mg l^{-1} on the y-axis but ppm in the caption and the text.
- The color of the DO saturation curve is orange rather than brown (adjust the caption).
- Mention in the caption the grey bar illustrating the ice cover and the meaning of thick lines (moving averages?).
- The depth of the DO sensor is 12 m in the caption, but it should be 10 m according to the methods (l. 122), which makes an important difference as the bottom low-oxygen layer starts below 12 m in Fig. 2a. The same depth of 12 m is also written in l. 484.
- Could you add temperature time series at the same depth as the DO data to highlight the effects of temperature oscillations on DO?
- There is a large peak at the end of the time series, with DO saturation reaching 130 %. Could it be due to mooring retrieval?

Figure 8:
- Indicate the periods of the two vertical lines associated with Kelvin waves, as the other periods. Why are there two peaks associated with Kelvin waves?
- The 12 h peak is not explained in the text (l. 381).

Figure 9:
- Indicate “mean lake depth” instead of “depth” in legend of (a), x-axis in (b) and caption.
- Indicate in the caption or on the y-axis of (b) the light extinction coefficient used in (a).
- Would it be possible to include the time series of modelled and observed surface temperatures in an additional panel? It would support l. 388-389.
- Mixing regimes are not explicitly indicated in (b) but discussed in l. 405-410. Would it be possible to add contour lines or another panel showing the two regimes as a function of mean lake depth and extinction coefficients? An option would be to use the number of mixing events during the year based on temperature differences of less than 1°C (l. 231).
- Changes in mixing regime mentioned in l. 397-398 should be moved later where the sensitivity analysis of Fig. 9b is described (l. 405).

Figure 10:
- The definition of h_{mix} is unclear: is it the deepest mixed layer of the year? Where do the values of h_{mix}/H come from?
- Better define the words used on the schematic. Is the stagnation layer the layer that remains stratified in summer? “Turbulence” is vague, why is it not present on the 3rd schematic?
- Are the dark and light grey shapes illustrating the location of the thermocline at two different times?
- Each schematic could be divided into 3 zones: fully mixed (white zone; O_2>100%), wave-affected = polymictic (sloping sides) and permanently stratified (bottom, O_2<100%).

7. The description of the annual stability (l. 330-347) based on Figure 6 could be inserted in the respective season-related subsections, instead of keeping it at the end of Sect. 3.2. and re-introducing the change in heat content in l. 331 (after having already described it in l. 292). Figure 6 could be combined with Figure 3.

8. The under-ice dynamics can be better described. The oscillations mentioned in l. 284-285 should be interpreted and a reference to Fig. 4 must be added. Explain where the under-ice convection (l. 287-288) is observed in Fig. 4 and mention the generation of internal waves as in l. 374. Could the temperature oscillations at 10 m before ice breakup also be due to differential heating? Correct l. 448-454 that state that under-ice convection is prevented in brackish lakes, which is inconsistent with the interpretation of Fig. 4.

9. The oxygen dynamics can also be better explained in Sect. 3.3. It would be useful to describe the oxygen stratification (less oxic bottom waters, Fig. 2a) at the beginning. The yearly high saturation mentioned in l. 350 might be specific to the sensor depth of 10 m, which seems to be above the bottom stratification under ice (Fig. 3a) and in summer (Fig. 2a). Hypoxia can occur in deeper layers and should be mentioned in l. 488. The oxygen drop after snowmelt (l. 358-361) is not explained: is it due to vertical mixing by under-ice convection?

10. References to previous studies should be added as follows:
    - l. 24-25: references for the list of endangered lakes.
    - l. 27-29: references about restoration measures.
    - l. 237-240: the first paragraph of the results is misleading as it starts with the salinity data that was already published in Andrulionis et al. (2022), it would be clearer to cite this source in l. 237 after "observational results". Is there a need to mention water sampling in l. 114-115 if this data is already included in Andrulionis et al. (2022)?
    - l. 209-217: the text is the same as in the model documentation (Mironov, 2005), which is not cited. Please rewrite these sentences instead of copying-pasting them.

    In addition, please check the references in the bibliography: a doi link is sometimes included twice for the same reference and it is sometimes absent, capital letters are sometimes incorrectly used in the article titles.

Minor comments:

- Units have a different font than other letters in the text.
- Font style and size vary between figures.
- Some sentences could be more concise to improve readability (e.g., l. 5-7, l. 30-36, l. 286-289, l. 444-447).
- There are many occurrences of “North Aral”. I would always use “Sea” after “Aral” to be consistent.
- l. 8: “annual” is a repetition of “year-long”.
- l. 10: “cold restart” is unclear, please give more information or replace by “restauration”.
- l. 13: ~4.5 days
- l. 16-17: dissolved matter (other than oxygen) and nutrients are not investigated in the study.
- l. 20 and l. 99: specify “1D modeling”
- l. 25: replace “threatening” by “causing” or “inducing”
- l. 29: replace “lake level replenishment” by “water replenishment” or “lake level increase”
- l. 63: remove “by this”
- l. 66: remove “conversely”
- l. 70: remove “there”
- l. 83: replace “to” by “for”
- l. 87: remove “in turn”
- l. 94, 95: no capital letter after (i) and (ii)
- l. 95 “periods of stagnation”, l. 96 “stagnation phases”, l. 101 “potential stagnation”: why not “stratification periods”?
- l. 97: replace “below” by “in this study”
- l. 99: remove “climate scenarios” since it is already part of modelling
- l. 108,110,117: not same precision of coordinates
- l. 110-111: the verb is missing (e.g., “...were performed”)
- l. 115: period missing at the end of the sentence
- l. 190: define “ODE”
- l. 225: define “ERA5”
- l. 256: repetition of “not”
- l. 273: “8 ‰” (space missing)
- l. 289: “referred TO as”
- l. 317: “Eq. (2)”
- l. 441: replace “preventing” by “delaying” since ice forms in winter
- l. 443: repetition of “the”
- l. 471: replace “turning the lake to polymictic” by “and could turn the lake to polymictic”, otherwise it could be interpreted as if the lake was polymictic.
- l. 491: ppm instead of ppt

References

Mironov, D. V. (2005). Parameterization of lakes in numerical weather prediction. Part 1: Description of a lake model. *German Weather Service*, Offenbach am Main, Germany. https://www.cosmo-model.org/content/model/cosmo/misc/flake/docs/ParLak_Part1_a.pdf