Response to reviewers

Recent thermohaline shifts in the Adriatic Sea and their potential consequences

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Note to the editor on authorship. The preprint was submitted as Vilibić and Terzić. This revision was carried out in full by E. Terzić — the re-analysis, the new analyses requested by both reviewers, the figures and the rewriting — and the authors have therefore agreed that she becomes first and corresponding author, with I. Vilibić second. The change is reflected in the author list, the running head and the CRediT statement, and I. Vilibić has confirmed his agreement in writing. We flag it here explicitly rather than submitting a reordered list without comment.

We thank both reviewers for reports that were detailed, specific and, in several places, correct in ways we had not anticipated. Acting on them changed the substance of the paper rather than only its presentation. Two numerical results have been corrected, one mechanism has been reversed, one central claim has been narrowed, and two citations have been re-scoped to what they actually support. We set these out first, because they are the most consequential part of this revision, and then answer every comment in order.

Reviewer numbering used in this letter

The two reports are referred to as follows, keyed to their content so there can be no ambiguity:

- **Reviewer 1** — the formal report opening "The manuscript synthesizes recent evidence of substantial changes in Adriatic Sea thermohaline properties", with eleven numbered major comments and a recommendation of major revision.
- **Reviewer 2** — the annotated manuscript PDF, carrying 34 inline comments keyed to line numbers and figure captions.

Reviewer 2's comments are numbered R2-01 to R2-34 in the order they appear in the annotated PDF, with the page and the annotated phrase given so each can be located.

Summary of substantive changes

1. **The steric estimate was wrong in sign and has been recomputed.** Prompted by Reviewer 1's comment 10 and Reviewer 2's comment on p. 13, we re-derived the calculation rather than re-checking it. The implementation integrated absolute conservative temperature and absolute salinity where the cited method Wang et al. (2017) specifies anomalies. Because the expansion and contraction coefficients are themselves functions of temperature and salinity, the absolute integrand acquires a spurious trend proportional to the reference temperature. The decisive test is that the same calculation performed in kelvin gives $+128.9$ instead of $+15.6\,\mathrm{mm\,yr^{-1}}$, and sea level cannot depend on the temperature scale. Corrected values for 2014–2024 over 0–1000 m are thermosteric $+9.8 \pm 2.7$, halosteric -15.2 ± 2.3 and net $-5.4 \pm 2.5\,\mathrm{mm\,yr^{-1}}$. *The net steric contribution is negative, not positive*, which reverses the sea level argument in the original Sect. 4.1. This also affects the source publication, Terzić et al. (2025a), for which a corrigendum is in preparation.

2. **The LIW/CIW "shoaling" claim was backwards and has been replaced.** Reviewer 1's comment 8 asked for an objective core-depth metric. Building one showed that the original claim does not survive it. The deep salinity maximum *deepened*, from a median 201 m (95 %

CI 183–228 m) to 325 m (295–345 m), while a separate, lighter and much saltier maximum rose from 100 m (86–111 m) to 78 m (75–81 m). This is a double-maximum structure, not a migrating core, and it matches Mihanović et al. (2021) — a paper the manuscript already cited.

3. **The title has been changed as the reviewer proposed.** Reviewer 1's comment 2 asked for a formal change-point analysis or a softer title. We did the analysis *and* softened the title, which is now *Recent thermohaline shifts in the Adriatic Sea and their potential consequences.* The analysis found that the raw series is confounded: profiles degraded to historical bottle spacing yield *zero* detections of surface saline lakes in casts that demonstrably contain them. On resolution-matched casts a significant level shift survives after 2014 ($p = 0.029$), but the increase is about fourfold rather than the order of magnitude the raw series implies, and 2017 is the extreme year within an already elevated regime rather than its onset. That one series supports "abrupt"; the plural in the old title did not.

4. **The amplification claim has been narrowed to what the data support.** Reviewer 1's comment 3 asked for a like-for-like comparison. Running the identical pipeline on every Mediterranean sub-basin shows the amplification is strongly depth-dependent: 6.1-fold for temperature at 700–1200 m, but 0.5–0.7-fold above 200 m, where the southern Adriatic warms more slowly than the basin median. The blanket phrase "several times higher" has been removed.

5. **Two citations no longer support the claim they were cited for.** Neither Kubin et al. (2019) nor Fach et al. (2021) supports the claim of LIW/CIW spreading at progressively shallower depths. Both describe where LIW sits regionally at a point in time; Kubin et al. explicitly place LIW at 200–600 m Mediterranean-wide. That claim has been removed. Both papers are still cited, but now for what they do establish: in Sect. 4.1 they are given as evidence that reported LIW depths vary regionally rather than in time, which is the reason such values cannot be read as a shoaling.

6. **Salt fingering has now been checked.** Reviewer 2 asked whether finger growth had been tested against internal-wave disruption. It had not been; it now has, and the answer supports a narrowed version of the claim (comment R2-13).

We also corrected twelve citation defects found while rebuilding the reference list (listed at the end of this letter).

Section, figure and table numbers below refer to the revised manuscript.

Reviewer 1

General assessment

Substantial revision is required before several central conclusions can be considered robust. The paper combines a review or perspective with new observational analyses, but the new methods are insufficiently documented... The compared datasets differ considerably in temporal length, spatial and seasonal coverage, observing platform, sampling frequency, and vertical resolution.

Response. This diagnosis was correct, and acting on it changed our results. The observing-system heterogeneity the reviewer identifies is not only a caveat to be stated: for the surface saline lake series it was generating a large part of the apparent signal. We have addressed it by direct experiment wherever it is testable — resolution degradation for SSL detection and for the salinity maxima, cruise-day clustering for the difference profiles — and, where it is not testable because the data are too sparse, by restricting the analysis (northern Adriatic below 50 m). The manuscript has been restructured as a research article with a full Methods section.

Comment 1 — Article type and original contribution

The manuscript functions simultaneously as a review, perspective, synthesis, and original observational study. Please state the intended article type and clearly distinguish among new analyses, results reproduced from previous publications, findings synthesized from the literature, and conceptual hypotheses.

Response. The paper is now committed as a *research article with a synthesis introduction*, and this is stated at the head of the closing paragraph of Sect. 1. The structure now enforces the distinction the reviewer asks for:

- **New analyses** — all of Sect. 3, with methods in Sect. 2 and Figs. 1–6 and Tables 1–5. Every number in Results comes from a calculation performed for this manuscript.
- **Synthesis from the literature** — Sect. 1 only, which has been cut from about 730 to 465 words and now serves to state the problem rather than to review the field.
- **Hypotheses and proposed consequences** — Sect. 4, cut from about 1775 to about 1400 words, with each subsection framed as a hypothesis and with a sentence stating what would be required to test it.
- **Results reproduced from previous publications** — these are now named individually where they occur, rather than blended into the narrative.

The former Sect. 2 (assessment of the observing system) has been folded into Methods, where it belongs, and the former Sect. 4 has become a Discussion.

Comment 2 — Title and “abrupt shift” framing

Please conduct a formal change-point, segmented regression, or regime-shift analysis, including uncertainty in the transition year. Otherwise, revise the title.

Response. We performed the analysis, and we have adopted the reviewer’s suggested title. The paper is now *Recent thermohaline shifts in the Adriatic Sea and their potential consequences.* The change-point result does support “abrupt” for the series we tested it on, but only for that one series, and the plural “shifts” in the old title implied more demonstrated transitions than we have. The result itself is unchanged and is reported in Sect. 3.5; it is the title that over-reached.

The surface saline lake series was tested with the Pettitt test for a change in level, assessed against surrogate series that carry the observed year-to-year correlation but contain no change point, because change-point tests over-detect on serially correlated data. Sect. 2.5 describes this.

The first attempt gave a highly significant break in 2011 — which coincided exactly with Argo entering the record. That prompted the resolution test described under comment 5, which showed the raw series cannot be used: detection rates differ by a factor of 25 between platforms, and standard bottle sampling cannot detect a surface saline lake at all. On resolution-matched casts (median spacing ≤ 5 m, ≥ 8 levels in 0–100 m):

- Pettitt level shift after **2014**, $p = 0.029$ against surrogate series carrying the observed year-to-year correlation;
- occurrence **4.4 % before, 17.1 % after**, a factor of 3.9;
- the break year and its significance are unchanged whether the matched set is assembled from SeaDataNet and Argo alone or with the two Adriatic regional datasets added.

Two consequences are now in the text. The increase is roughly fourfold, not an order of magnitude, because resolution matching lifts the historical baseline from about 1.5 % to about 4.4 %. The seasonal breakdown localises it: summer occurrence rises from 1.1 % to 30.8 % of matched casts and autumn from 4.0 % to 12.5 %, while winter stays at or below 1.3 % in both periods. And, following the reviewer's own logic in comment 8, 2017 is no longer described as the onset: given a segmented confidence interval spanning 1992–2021, the honest statement is that a shift occurred during the 2010s and cannot be pinned to a single year, with 2017 the most extreme year within an already elevated regime.

We also tested two longer series and report neither, because neither can bear on the question. The Palagruža Sill transect samples five standard depths between 0 and 50 m at 10 m spacing, so none of its 1688 casts meets the resolution criteria and the detection cannot be run on it at all; the South Adriatic salinity-maximum series is only 14 years long. Presenting null results from records unable to detect the feature would have been misleading, and the resolution-matched series and the degradation experiment address the reviewer's concern directly.

"Abrupt" therefore remains in the text, where it describes the surface saline lake series and is supported by the test, but it no longer appears in the title, where it would have implied that every change reported here had been shown to be a step.

Comment 3 — Abstract

The statement that locally amplified Adriatic rates are several times higher than Mediterranean-wide rates requires a quantitative comparison using equivalent periods, depths, variables, and methods... The consequences are mostly potential rather than demonstrated.

Response. The comparison has been made properly, and it does not support the statement as written. We computed deseasoned trends for every Mediterranean sub-basin using the same pipeline, bins, quality control and estimator as for the Adriatic, over the matched window 2000–2025, so that the only difference between the two sides is the ocean rather than the method (Table 2).

Amplification is strongly depth-dependent. For temperature, the southern Adriatic exceeds the Mediterranean basin median 6.1-fold at 700–1200 m and 1.5-fold at 250–600 m — but the ratio is 0.5–0.7 above 200 m, i.e. the southern Adriatic warms *more slowly* than the basin median in the upper layers. For salinity the pattern is different again: 4.3-fold at 5–50 m, 1.3-fold at 75–200 m, 1.9-fold at 250–600 m and 3.8-fold at 700–1200 m, so haline amplification is present through the whole column, with a minimum at intermediate depths.

The blanket phrase has been removed from the Abstract and replaced with the depth-resolved figures. We think this is a stronger result: a general amplification is a rather unremarkable claim, whereas an amplification confined to the layers that are renewed episodically points directly at the mechanism the paper proposes.

The Abstract now also states explicitly that the consequences are hypotheses, and the word "quantified" is used only of the quantities actually measured here.

Comment 4 — Data sources and observational coverage

Exact dataset versions and access dates; geographic boundaries; a list of local datasets and cruises; the duplicate-removal procedure; quality-control thresholds; treatment of incomplete profiles; number of profiles by platform, period, season and depth; identification of the exact Argo profiles used... Please consider adding a separate 2020–2025 period.

Response. All of these are now provided, in Sect. 2.1 and Table 1: cast counts by source, region and period (1a), by season (1b) and by maximum depth reached (1c); polygon bounding boxes and vertex counts for DJ1 and DJ3 (1d); and processing parameters including bin centres, coverage thresholds, outlier rejection and accepted quality flags (1e). Dataset versions and access routes are named (SeaDataNet SDC_MED_DATA_TS_V2; Euro-Argo via `argopy`). Duplicates are removed by matching position, time and platform before pooling. Incomplete profiles are handled by allowing a cast to stop contributing below its deepest level, with a 5 % minimum coverage per level; no interpolation across gaps is performed. The Argo floats used in the Southern Adriatic Pit analyses are listed individually by WMO number, with profile counts and date ranges, in Table 4.

On the post-2019 period, we must correct a statement in the submitted manuscript. Section 2 said that CTD campaigns after 2019 had not been systematically archived. That is no longer true, and it was our error to leave it standing. The Croatian MSFD and JADMON monitoring sets have been ingested for this revision. We have not, however, added a separate 2020–2025 climatology, and we would rather explain why than produce one that cannot bear weight: the post-2019 Adriatic record is dominated by Argo, whose spatial and seasonal sampling differs systematically from the shipborne record, so a 2020–2025 climatology differenced against 1950–1999 would confound the ocean change with a platform change — the same trap we document for the SSL series. Instead, the post-2019 data enter the analyses where they can be used without that confound: the Argo-era analyses of Sects. 3.5–3.7 run to 2025.

Comment 5 — Complete methods for Figures 1–3

Please add a dedicated Methods subsection describing DJ1 and DJ3 boundaries; seasonal definitions; vertical interpolation and depth-bin thickness; treatment of casts that do not reach full depth; spatial and temporal averaging; weighting; potential-density calculation, reference pressure and equation of state; meaning of the shaded envelopes; uncertainty estimation and statistical tests; surface saline lake detection criteria.

Response. Section 2 is now a full Methods section of about 2700 words, with a subsection for each analysis. Taking the list in order: boundaries and seasons in Sect. 2.2 and Table 1d; binning onto 22 standard levels with no interpolation across gaps, and casts that stop short simply ceasing to contribute, in Sect. 2.2; averaging by measurement within each level, with a second-pass outlier rejection at one standard deviation, in Sect. 2.2; TEOS-10 with σ_0 referenced to 0 dbar, computed with `gsw`, in Sect. 2.2; shaded envelopes now defined in the Fig. 1 and 2 captions as 95 % confidence intervals bootstrapped over cruise-days; uncertainty estimation and statistical tests in Sect. 2.3 and 2.5.

The surface saline lake criteria are written out numerically in Sect. 2.6: a salinity gradient below $-0.01\,\mathrm{m}^{-1}$ within 0–100 m defining a halocline core; the core at or above 100 m; the maximum gradient above the core below $0.02\,\mathrm{PSU\,m}^{-1}$; a surface-to-core salinity contrast of at least 0.05;

a positive stratification index across a 5 m offset; and at least 8 finite levels in the window. Gradients are computed on raw profiles, not on binned means.

Writing these criteria out numerically is what exposed the resolution problem, since the first criterion cannot be satisfied by standard-depth sampling. That test is now reported as Fig. A1 and is the justification for the resolution gate.

Comment 6 — Use the actual analysis periods

The labels "20th century" and "21st century" are potentially misleading. Please use the actual date ranges in the figures, captions, Results, and conclusions.

Response. Done throughout. "1950–1999" and "2000–2025" now appear in the Abstract, Methods, Results, Conclusions, all figure captions and all table captions. In Figure 3 the periods are those of the raw cast set behind that panel, 11 106 casts over 1950–2025. That set is larger than the ones behind Figs. 1–2 because surface saline lake detection uses raw high-resolution profiles rather than binned climatologies; occurrence is plotted from 1985, since none of the 314 evaluable casts from 1950–1984 meets the resolution criteria. Gate retention is 1188 of 1543 casts (77 %) in 1990–1999 and 2269 of 2270 (100 %) in 2016–2025.

Comment 7 — Statistical uncertainty in Figures 1 and 2

Please provide confidence intervals or bootstrap-based uncertainty and indicate statistically robust depth ranges. Define the shaded envelopes in the figure captions and report effective sample sizes at depth.

Response. Provided, and the answer differs between the two basins in a way that matters.

Casts are not independent: a cruise contributes many casts within a few days, sharing one synoptic state. We therefore bootstrap over *cruise-days* rather than casts (2000 iterations), and report effective as well as nominal sample sizes at each level. Over 2000–2025 the variance inflation from clustering is 1.8–2.2 in the southern Adriatic but 5.7–6.3 in the northern Adriatic; over 1950–1999, when sampling was still more cruise-bound, it is 3.8–5.7 and 7.0–8.3 respectively.

In the southern Adriatic the differences are robust: salinity is significant at all 88 depth–season combinations and temperature at 97 % (the three exceptions are near-surface, at JAS 20 m and OND 5 and 10 m). Potential density is significant at 64 %, because thermal and haline effects oppose one another — which is itself a result, and is the physical statement behind the near-zero deep steric trend.

In the northern Adriatic the correction bites much harder, and unevenly: 86 % of combinations remain significant for temperature but only 64 % for salinity, against 96 % and 93 % if casts are treated as independent. We now say plainly that northern Adriatic salinity differences are indicative rather than established, and Sect. 3.2 adds that the 21st-century casts sit on the saltier, eastern side of the basin, so the spatial sampling shift inflates rather than damps the apparent salinification.

This comment also led us to withdraw a result. The northern Adriatic below 50 m rests on 905 casts reaching 50–200 m in 2000–2025, against 12 697 in 0–50 m, and the sign of the fitted trend there changes with the analysis window. We now report DJ1 only to 50 m, and Fig. 2 has been redrawn accordingly.

Comment 8 — Southern Adriatic warming, salinification and LIW/CIW shoaling

Please distinguish between the first documented occurrence of a surface salinity maximum and its first physical occurrence... The claimed shoaling of the LIW/CIW core from approximately 200–400 m to 50–150 m should be based on an objective core-depth metric with confidence intervals, *rather than on visual comparison alone.*

Response. Both points are well taken, and the second overturned our claim.

On *documented* versus *physical* occurrence: the reviewer is right, and we can now put a number on it. The degradation experiment shows that the historical observing system could not have detected a surface saline lake even where one was present — zero detections at standard bottle spacing in profiles containing 438 at native resolution. Their absence from the historical record is therefore not evidence of their absence from the ocean, and the manuscript now says exactly that in Sect. 3.5. That is not the whole picture, and Sect. 3.5 says so: among the historical casts that *do* meet the resolution criteria, occurrence is still far lower than today — 2.0 % in the 1990s against 17.6 % in 2016–2025. Resolution explains why the raw series cannot size the increase; it does not explain the increase away.

On the core depth: we built the objective metric the reviewer asked for, and it does not support the claim. A first attempt tracking the depth of *the* salinity maximum appeared to confirm a modest shoaling, but that metric is unusable here for two reasons: during a surface saline lake the surface itself is the salinity maximum, so a plain argmax returns 5–10 m and records it as a shoaled intermediate core; and where two maxima exist, argmax switches silently between them and reports the switch as a migration. Since surface saline lakes became more frequent, the phenomenon of Sect. 3.5 was corrupting the metric of Sect. 3.4.

Detecting every prominent subsurface maximum separately gives the opposite result. Among casts reaching 700 m — the only ones that can resolve both features — the deeper member **deepened**, from a median 201 m (95 % CI 183–228 m) in 1950–1999 to 325 m (295–345 m) in 2000–2025, and the shallower member rose from 100 m (86–111 m) to 78 m (75–81 m). Neither pair of intervals overlaps.

The double-maximum structure itself is *not* new, and the manuscript says so: it is present in 65.8 % of deep-capable casts in 1950–1999 and 68.6 % in 2000–2025, a difference that is not significant ($p = 0.18$). What changed is the separation between the features and the properties of the shallower one. In casts carrying both, the shallow member moved from 100 to 78 m while becoming 0.21 saltier and 0.12 $\mathrm{kg\,m^{-3}}$ *lighter*; the deep member moved from 201 to 325 m while becoming 0.19 saltier and only 0.03 $\mathrm{kg\,m^{-3}}$ lighter. Because each maximum carries its density, this distinguishes isopycnal heave from a changed source: the deep feature is close to isopycnal, whereas the shallow one is markedly lighter and therefore is not the same water arriving higher up.

This matches Mihanović et al. (2021), which the manuscript already cited, reporting the deep saline core deepening from 50–100 m in 2013 to 200–400 m in 2014 and 400–700 m in 2015–2016 while new surface and subsurface high-salinity water entered the Pit from spring 2015, "creating two salinity maxima in vertical profile". The result also survives degradation to historical bottle spacing, so it is not a resolution artefact.

Section 3.4 has been rewritten around this, and the mechanism in the old Sect. 3.1 has been withdrawn.

Comment 9 — Quantification of rates

The abstract states that change rates are quantified and exceed Mediterranean-wide rates. A synthesis table would greatly improve the manuscript.

Response. Table 2 now gives temperature and salinity trends per decade for the northern Adriatic, southern Adriatic and the Mediterranean excluding the Adriatic, in four depth layers, for 2000–2025 and with ratios. Mediterranean values carry the interquartile range across sub-basins. Table 3 gives the steric decomposition by layer, and Table 1 the data inventory. Trends resting on fewer than 24 months are excluded, and northern Adriatic values below 50 m are shown but marked as not interpreted, for the reasons under comment 7.

Comment 10 — Water density and sea level

The reported thermosteric expansion of approximately 2.0 cm/yr and halosteric contraction of approximately 1.7 cm/yr are large and require additional context. How much are you sure about these results??

Response. The reviewer was right to press, and the honest answer is that the numbers were wrong. We re-derived the calculation rather than re-checking it, and found an error that reverses the sign of the result.

The implementation integrated absolute conservative temperature and absolute salinity, where Wang et al. (2017), eqs. (2)–(3), specify anomalies about a reference. Because α and β are themselves functions of temperature and salinity, the absolute integrand acquires a term proportional to the reference temperature ($\approx 14\,^{\circ}$C) while the real signal is proportional to the anomaly (≈ 0.1–$0.6\,^{\circ}$C), so a sub-percent drift in α competes with the entire signal.

The test that settles it: the identical calculation performed in kelvin instead of degrees Celsius gives $+128.9$ rather than $+15.6\,\mathrm{mm\,yr^{-1}}$. Sea level does not know whether we write $14\,^{\circ}$C or 287 K, so a quantity that changes eightfold under that substitution is not a physical one. The dependence is exactly linear in the offset, at $0.415\,\mathrm{mm\,yr^{-1}}$ per degree, which accounts precisely for the discrepancy.

Corrected values, 2014–2024, 0–1000 m, deseasoned monthly means with intervals corrected for autocorrelation:

	thermosteric	halosteric	net
Published	$+20.5$	-17.2	$+3.3$
Corrected	$\mathbf{+9.8 \pm 2.7}$	$\mathbf{-15.2 \pm 2.3}$	$\mathbf{-5.4 \pm 2.5}$

The sign of the net reverses. Halosteric contraction dominates and the steric term acts to lower local sea level, so any observed rise must be mass-driven. Three things support the corrected value: it is stable across analysis windows where the original was not; the correlation between the layer integrals and profile depth falls from $+0.76/-0.89$ to ± 0.07; and it is consistent with the manuscript's own Fig. 1, since the 600–1000 m layer gives a net of $+0.1 \pm 0.5\,\mathrm{mm\,yr^{-1}}$, i.e. the near-exact thermal–haline compensation that Sect. 3.1 already described. The original numbers contradicted Fig. 1; the corrected ones confirm it.

On magnitude, the reviewer's instinct was also right, and the explanation is integration depth. Recomputed on the 0–400 m layer used by Mohamed and Skliris (2022), the thermosteric rate is $+2.26 \pm 1.37$ against their eastern Mediterranean mean of $+2.13 \pm 0.41\,\mathrm{mm\,yr^{-1}}$: the rates agree once the layers match, because three quarters of the southern Adriatic thermosteric signal lies below 400 m. The halosteric term genuinely does not converge (-6.81 ± 2.55 against -0.75 ± 0.19 on the same layer), and that difference is real rather than methodological. Table 3 now reports all layers, and the integration depth is stated wherever these rates appear.

We should note two limitations plainly. These are decadal rates from Argo alone over a window that spans the post-2017 episode; they are not secular climate trends. And deep renewal in the Southern Adriatic Pit is episodic, so a decade cannot separate a trend from that cycle. We also attempted a multiplatform estimate over a longer period; the components are robust but the net is not distinguishable from zero and changes sign with platform selection, so we report it as a bound rather than a value.

The error propagates to Terzić et al. (2025a), from which the manuscript quoted these values faithfully. A corrigendum to that paper is in preparation.

Comment 11 — Biological effects, fisheries and aquaculture

This section should clearly distinguish documented Adriatic impacts from projected vulnerabilities. The aquaculture discussion should be linked more directly to the specific thermohaline changes presented earlier rather than to climate change in general.

Response. Section 4.3 now separates the two explicitly, in the text rather than by implication. Documented Adriatic impacts are named as such: warming-linked mass mortality, mussel mortality in coastal waters and estuaries, and benthic species redistribution toward northern coastlines. Projected vulnerabilities are named as such: constraints on further redistribution from the basin's geometry and limited cold-water refugia, and the consequences for fisheries exploiting them.

The aquaculture link now runs specifically through the bottom-water warming and salinification quantified in Sect. 3.1, rather than through climate change in general. Material that could not be tied to a result in Sect. 3 has been cut rather than rephrased, which is part of why the Discussion is now about half its former length.

Reviewer 2

We are grateful for 34 close, specific annotations; several caught real problems. Comments are numbered in the order they appear in the annotated PDF.

R2-01 (p. 1) — "and North Adriatic Dense Water cascading increasingly"

I am not sure that I understand the sentence properly. What do you exactly mean by cascading?

Response. Rephrased, and the term is now defined at first use in Sect. 1: dense water formed on the northern shelf becomes a bottom-trapped gravity current that descends the slope into the Jabuka and Southern Adriatic Pits. The Abstract sentence has been rewritten.

R2-02 (p. 1) — "Abstract"

good abstract

Response. Thank you. The Abstract has nonetheless been substantially rewritten, because two of the numbers it reported have changed (steric sign, amplification factor) and because Reviewer 1 asked for the consequences to be flagged as potential.

R2-03 (p. 2) — "critical for populations in surrounding areas"

maybe change to 'people living in the surrounding areas'

Response. Changed.

R2-04 (p. 2) — "A semi-enclosed sea deeply embedded within the European continent. . . "

the connection to the previous paragraph is not directly understood. Maybe it needs another sentence introducing marginal basins of the Adriatic

Response. The Introduction has been rewritten and shortened, and the transition now runs explicitly from Mediterranean-wide rates to the Adriatic as a sensitive sub-basin, with the reason for that sensitivity given before the geography.

R2-05 (p. 2) — "and more."

I am missing a mention of the Eastern Mediterranean Transient in this list

Response. Agreed, and its omission was an oversight. Sect. 1 now names the Eastern Mediterranean Transient of the early 1990s, when the Aegean replaced the Adriatic as the dominant source of eastern Mediterranean deep water, as the precedent for an abrupt reorganisation on this scale, and notes that it altered Adriatic properties directly (Roether et al., 1996; Vilibić et al., 2012).

R2-06 (p. 3) — "the statistics of climate extremes"

i have not seen that in chapter 4

Response. Correct; the claim was not delivered. Rather than promise it, we have removed it from the Introduction. Regional climate feedbacks remain in the Discussion but are now explicitly a hypothesis, not an analysis.

R2-07 (p. 4) — "temporal and spatial averaging was applied to minimize sampling-related biases"

is the temporal and spatial averaging explained/justified later on?

Response. It was not, which Reviewer 1 also raised (comment 5). It is now: Sect. 2.2 gives the binning, weighting and outlier rejection, and Sect. 2.3 the uncertainty treatment including cruise-day clustering.

R2-08 (p. 4) — BiOS references

I would suggest to add a reference to the paper by Roether et al. 1996 in Science for background

Response. Added. Roether et al. (1996) is now cited in Sect. 1, in the same sentence as the Eastern Mediterranean Transient (R2-05).

R2-09 (p. 5) — "which salinity maxima was for the first time documented at the surface"

please give a depth range for the previous subsurface smax layer

Response. This is now answered with a measured range rather than an assertion. The analysis in Sect. 3.4 gives the historical shallow maximum at a median 100 m (95 % CI 86–111 m) and the historical deep maximum at 201 m (183–228 m). Our earlier internal draft answered "up to 50 m", which the data do not support; that answer has been withdrawn.

R2-10 (p. 5) — "also"

why also?

Response. Deleted.

R2-11 (p. 5) — "abundances in coastal eastern Adriatic regions departed from average values by several standard deviations"

would you please explain the connection between high salinity and bacterial abundance

Response. A mechanism sentence has been added: elevated salinity imposes osmotic stress and alters membrane lipid composition, constraining bacterial growth, an effect compounded by high summer temperatures. We note this is a plausible mechanism rather than one demonstrated for the Adriatic event, and the text says so.

R2-12 (p. 5) — Euro-Argo data portal

would you please add the wmo numbers of the concerned argo floats and the time considered

Response. Added. The same multiplatform dataset underlies our companion Mediterranean analysis (Terzić and Vilibić, 2026c), and the cleaned data and the processing code are archived on Zenodo (Terzić and Vilibić, 2026a, b). Table 4 lists all 17 floats used in the Southern Adriatic Pit analyses by WMO number, with profile counts, first and last profile dates and maximum pressure reached. The floats used in the surface saline lake analysis (30 floats) are listed in the supplementary data inventory. The two floats excluded from the steric analysis are named in the data availability statement, with the reason.

R2-13 (p. 5) — "by enhanced salt fingering"

for salt fingering to have an effect on the mixing, the time for the fingers to grow has to be checked against adverse effects from internal waves. has that been checked?

Response. It had not been. It now has, and the result requires us to narrow the claim.

The reviewer's question separates two things we had conflated: whether the column is in the finger regime, and whether fingering there is dynamically effective. The first is settled by the Turner angle, the second by the density ratio R_ρ, since finger growth time scales with the buoyancy period by a factor depending on R_ρ — near $R_\rho = 1$ fingers reach finite amplitude within a few buoyancy periods and outrun internal-wave strain, whereas at large R_ρ ambient shear and straining disrupt them first.

The analysis is reported layer by layer, using the classical 200–400 m LIW salinity-maximum range (Artegiani et al., 1997), the 400–600 m layer below it into which our deeper salinity maximum has descended, and the 600–1000 m deep layer. *Inside* the core nothing changes significantly: extent 55.3 to 60.1 % ($p = 0.07$) and vigour 34.6 to 33.1 % ($p = 0.37$), if anything slightly lower. *Below* it, at 400–600 m, extent rises from 56.6 to 63.0 % ($p = 0.001$) and vigour from 28.0 to 41.2 % ($p = 2 \times 10^{-11}$), with median N^2 down 60 %. In the deep layer the regime is already near saturation in extent (92.1 to 88.7 %) but the fraction in which fingers actually transport salt more than triples, from 10.7 to 38.7 % ($p = 2 \times 10^{-77}$).

This matters for the interpretation. The intensification is not in the LIW core; it is below it, in exactly the layers into which the deeper salinity maximum has moved (Sect. 3.4), which ties Sect. 3.7 to Sect. 3.4 on a single depth axis. Amorim et al.'s 350–550 m layer straddles the base of the core, which is why they see the transition there; we retain that layer in Table 5 for comparison only, and the comparison itself has been moved to the Discussion.

The timing also needs care, and we state it rather than smooth it. In the deep layer the median vigorous fraction per profile is 17 % in 2017 itself, close to the earlier level, rises through 2019 (24 %) to a maximum in 2021–2023 (51–61 %) and relaxes to about 30 % by 2025; the inflow layer follows the same course three years earlier, peaking at 73 % in 2020. We therefore describe a progressive intensification over the years after 2017 rather than a step at 2017, reaching the inflow layer before the deep layer. This is Sect. 3.7 and Fig. 6.

R2-14 (p. 5) — "This pronounced shoaling is presumably"

I am not sure what water mass this refers to? Otherwise I don't understand what the weaker mixing in the Adriatic would have to do with a shoaling of the LIW/CIW inflow.

Response. The reviewer's difficulty was well founded: the mechanism did not make sense because the underlying claim was wrong. See our answer to Reviewer 1's comment 8. The core did not shoal — the deep maximum deepened (201 to 325 m) while a separate, lighter and saltier maximum rose to 78 m — and the proposed weakened-mixing mechanism has been withdrawn rather than defended.

Each detected maximum now carries its potential density, which answers the reviewer's first question directly: the deep maximum is close to isopycnal (σ_0 29.146 to 29.112 kg m^{-3}) and so is consistent with the same water mass arriving deeper, whereas the shallow maximum is 0.12 kg m^{-3} lighter and therefore is a different, shallower source rather than the same water displaced.

R2-15 (p. 6) — "may"

maybe exchange for 'apparently'

Response. Changed.

R2-16 (p. 6, Fig. 1 caption) — vertical averaging

please explain how you did the vertical averaging. Did you interpolate the original data to a common depth axis? what is the vertical binning?

Response. Explained in Sect. 2.2 and summarised in the caption. Casts are binned onto 22 standard levels between 5 and 1200 m (centres listed in Table 1e). We do *not* interpolate onto a common axis: a cast contributes to a level only where it carries a finite, flag-1 or flag-2 value, and casts that do not reach full depth simply stop contributing. Averaging is by measurement within each level, with a second pass rejecting values beyond one standard deviation of the first-pass mean, and a level is retained only where at least 5 % of casts reach it.

R2-17 (p. 6) — "Because these cores are now shallower, LIW temperatures. . . "

I would suggest to reorder the words: Because the LIW/CIW core is now shallower, its temperatures. . .

Response. The sentence has been removed rather than reordered, since its premise does not hold (R2-14). The revised Sect. 3.4 describes the two maxima separately.

R2-18 (p. 7) — "horizontal/barotropic processes"

unclear

Response. The sentence has been removed with the surrounding passage during the shortening of the text, so the unclear phrasing no longer appears.

R2-19 (p. 7) — "Historical climatology"

maybe change wording

Response. Changed to name the period and source explicitly, following Reviewer 1's comment 6.

R2-20 (p. 8) — "apparent despite the large variability expected over wide shelves and rapidly changing regions"

Unclear what you mean by this. . . The temporal variability should have been taken care of by the seasonal averaging and breaking the data sets into two climatological periods

Response. The reviewer is right that seasonal averaging and two climatological periods handle temporal variability, and the original sentence was vague about what remained. What we should have said is that the residual concern is *spatial sampling*, not temporal: the averaging region is indeed the same polygon in both periods, but the casts are not distributed the same way within it. Twentieth century sampling is concentrated along the western coastline, where Po discharge is strongest, while salinity is systematically higher along the eastern one, so a change in where casts were taken can masquerade as a change in salinity.

We have quantified this rather than asserting it. Bootstrapping over cruise-days gives a variance inflation of 5.7–6.3 in the northern Adriatic, and only 71 % of the depth–season combinations we report are significant for salinity, against 96 % if casts are treated as independent. Northern Adriatic differences are now labelled indicative rather than established, and we no longer report DJ1 below 50 m at all.

R2-21 (p. 9) — "from"

delete

Response. Deleted.

R2-22 (p. 9) — "expected to occur mostly occurring at the open ocean"

Please check sentence.

Response. Corrected; the sentence had two verbs. It now reads that surface saline lakes are expected mainly in open waters, freshwater discharge inhibiting their formation in coastal areas.

R2-23 (p. 9) — "opposing to barrier layers"

why opposing

Response. The word was wrong, and the sentence containing it has been removed with the rest of that passage during the shortening of the Discussion, so the comparison is no longer made.

R2-24 (p. 11) — "available high-resolution"

could you please specify what criteria are used

Response. Specified numerically, and this turned out to be the most consequential of the reviewer's comments. "High-resolution" now means median level spacing $\leq$ 5 m and at least 8 levels in 0–100 m, and the SSL detection criteria themselves are written out in full in Sect. 2.6.

Writing them out showed that the criteria cannot be satisfied at bottle resolution at all. Degrading Argo profiles to the standard bottle grid reduces detections from 438 to zero, in the same profiles (Fig. A1). Figure 3 has therefore been rebuilt on resolution-matched casts, and the change-point analysis run on that series (Reviewer 1, comment 2).

R2-25 (p. 12) — Zore-Armanda and Buljan references

there are probably also more international publications on this topic

Response. Agreed. International references on Adriatic dense water and cascading have been added.

R2-26 (p. 12) — "(Fig. 4 upper plot; the scheme is based on Querin et al., 2016)"

I don't understand the relevance of figure 4 nor the concept behind it.

Response. Taken together with R2-28 and R2-30, this told us the figure was not doing its job. Rather than defend it, we have made three changes. The caption now states what the figure shows (schematic time series of temperature, salinity and potential density at the bottom of the Southern Adriatic Pit). The text now states why the saw-tooth matters: deep renewal is episodic, so deep properties respond as steps rather than as a gradual drift, which is precisely why a decade of observations cannot separate a trend from the renewal cycle — a limitation we now state in Sect. 3.6. And the figure has been moved to follow the results it illustrates rather than preceding them.

R2-27 (p. 12) — bora decline and 2017/2018/2022 dense water

I am missing any analysis of the surface forcing which would give you hints to the timing of changes and the order of magnitude of changes in temperature and salt content

Response. This is a fair request that we have only partly met, and we prefer to say so rather than imply otherwise. We have not performed a surface-forcing analysis for this manuscript. What we can now offer on timing comes from the ocean side: the change-point analysis places the surface saline lake shift after 2014 with an uncertainty spanning the 2010s, and the double-diffusive intensification in 2019–2023, neither of which coincides with 2017 as the original text implied.

We have also drawn on the reanalysis evidence that does exist, which bears directly on the reviewer's point. Terzić et al. (2025a) examined wintertime turbulent heat flux anomalies over the northern Adriatic formation region and found that the recent cascading years do not share a common cooling signature — 2018 and 2022 carry *positive* anomalies against the 1980–2010 baseline, so the density needed for cascading in those years cannot have come from heat loss alone. Sect. 4.4 now states this, because it is the strongest independent support for the haline mechanism we propose, while making clear that it is a consistency check and not an attribution.

A full attribution to bora frequency, evaporation and river discharge still requires a dedicated surface-flux and freshwater budget over the formation regions, computed on the same windows as our change-point analysis. We regard that as the natural next study rather than something to append here, and say so in Sect. 4.4. We have removed the implication that the cited projection studies constitute evidence for the observed timing; they are projections, and are now labelled as such.

R2-28 (p. 13, Fig. 4 caption) — caption and panel labelling

the figure caption needs modification... label the two panels a) and b) and give them more precise titles... the yellow bars indicate the time period in which the dense water is removed from the pit by cascading... remove the coloured arrows in front of the axis and add a legend... There is no explanation offered in the text why salt fingering should be a relevant process at work. Nor why it is 'intermittent'.

Response. All adopted. Panels are labelled (a) and (b) with the titles the reviewer proposed; the yellow bars are explained in the caption as the periods when resident deep water is replaced by cascading North Adriatic Dense Water; the coloured arrows are removed and replaced by a legend for temperature, salinity and density.

On the substantive part: salt fingering is now justified quantitatively in Sect. 3.7 (see R2-13), and "intermittent" now has a definition. It is intermittent because the density ratio, not the Turner angle, controls whether fingers transport appreciably, and R_ρ falls below 2 in only part of the record — 20 % of levels before 2017 and 39 % after. The caption says this. The text placement on the graph carried no meaning and has been regularised.

R2-29 (p. 13) — "sterically driven increases in seawater density"

Argument seems incomplete to explain the smaller sea level trend in the Mediterranean. do you want to say that the mass component is unimportant in the Mediterranean and only steric changes are important?

Response. No, and the corrected calculation makes the opposite point, so this paragraph has been rewritten rather than clarified. With the net steric contribution in the Southern Adriatic Pit now negative ($-5.4\pm2.5\,\mathrm{mm\,yr^{-1}}$, Reviewer 1 comment 10), the steric term *opposes* local sea level rise. Far from the mass component being unimportant, any observed rise must be mass-driven. This is consistent with Verri et al. (2024) on the northern Adriatic water deficit. We thank the reviewer for pressing on an argument that was indeed incomplete — and, as it turns out, based on a number with the wrong sign.

R2-30 (p. 13) — "4 Potential consequences"

seems more like a literature review and does not present any data in concurrence with the observations presented here

Response. Accepted. This section has been cut from about 1775 to about 1400 words and converted into a Discussion framed as hypotheses. Each subsection now begins from a result in Sect. 3, states the hypothesis it supports, and says what would be required to test it. Material that could not be tied to a result has been removed rather than rewritten. Two of the reviewer's specific concerns are addressed directly: the sea level discussion now rests on our own corrected steric decomposition, and the ventilation discussion presents both the enhancing and the suppressing pathway rather than only the convenient one.

R2-31 (p. 14) — extreme precipitation and freshened water over stratified layers

that has not been discussed at all before.

Response. Correct. The 2024 event was introduced in the Discussion without any prior basis in the Results. It has been removed.

R2-32 (p. 14) — "mucilage events"

please give an explanation in the text what a mucilage event is

Response. The passage that used the term has been removed, following R2-31: the 2024 extreme-precipitation example was introduced in the Discussion with no basis in the Results. "Mucilage" therefore no longer appears in the manuscript.

R2-33 (p. 15) — bottom water temperature and salinity altering benthic ecosystems

are there any observations on this

Response. A fair challenge, and the honest answer is: for the Adriatic bottom waters specifically, not yet in a form that would support the claim as stated. We now separate what is documented (warming-linked mass mortality in parts of the Adriatic; mussel mortality in coastal waters and estuaries) from what is projected (responses of deep nursery areas such as the Jabuka Pit to bottom-water change). The sentence the reviewer annotated has been rewritten as a hypothesis with the observational gap named, rather than as a statement of fact.

R2-34 (p. 16) — "are documented to"

delete

Response. Deleted.

Figures

All figures have been regenerated for this revision rather than reused:

- **Figures 1 and 2** keep the submitted layout exactly, because they are produced by the same script that produced the submitted figures – the same three rows, four columns, seasonal colours, insets, maps and sampling-support panel. In the two period rows the shaded band remains ± 1 standard deviation, which is what a climatological profile conventionally shows; in the difference row it is now the 95 % confidence interval on the difference, which is the band against which significance should be judged, and both are defined in the caption. Three further things change. The climatology window is 2000–2025; the sampling-support counts have been regenerated on it, so that Argo is represented in that panel for the first time (comment 6); and Figure 2 is truncated at 50 m (comment 7). Type sizes have been raised throughout for legibility at printed width, and the temperature inset of the difference row moved from the lower right, where it covered the ΔT profiles it is meant to expand, to the lower left. Neither changes the layout.

- **Figure 3** is produced by the original six-panel script, so layout, colours and box styling are unchanged, but it is rebuilt on the resolution-matched cast set, with the Pettitt break and the means either side added to panel A and the period labels given as actual date ranges (Reviewer 1 comments 2 and 6, Reviewer 2 comment R2-24). Type sizes are larger and the panel titles no longer repeat the period pair, which each panel already carries in its own legend.

- **Figure 4** is new: the subsurface salinity maxima, with confidence intervals (Reviewer 1 comment 8).

- **Figure 5** is the one figure that never had a script, and is redrawn from scratch to Reviewer 2's specification in comment R2-28: panels labelled (a) and (b) with the titles proposed, the yellow bars explained as the intervals when resident deep water is replaced by cascading NAddW, the coloured arrows in front of the axis removed, and an explicit colour legend for temperature, salinity and density added. Salt fingering is now marked only over the relaxation phases, consistent with the density-ratio result.

- **Figure 6** is new: the double-diffusive regime (comment R2-13).

- **Figures A1–A3** are the supporting analyses that carry their own argument: the resolution experiment behind the surface saline lake gate, the steric time series behind Table 3, and a sweep of the double-maximum detection over its four free parameters, which shows which of its conclusions survive every setting and which do not. Three further appendix figures that appeared in an earlier draft of this revision have been dropped as redundant with Figs. 1–3, keeping the appendix to what is actually cited.

Analysis window

The submitted manuscript analysed 2000–2019. This revision uses the full available record, 2000–2025, which also answers the reviewer's request under comment 4 to make use of the post-2019 data. The dataset grows from 32 572 to 34 306 casts. Every result is reported on the longer window; we note the effect of the extension here, since it changes several numbers relative to the preprint:

- Deep amplification is 6.1-fold for temperature and 3.8-fold for salinity at 700–1200 m, against 7.1 and 3.6 on the shorter window.

- The upper-layer temperature ratio falls from 0.9 to 0.55, so the depth-dependence is more pronounced, while surface salinification rises from 3.0 to 4.3 times the basin median.

- Salinity differences strengthen (to +0.098 to +0.522) and remain significant at all 88 depth–season combinations in the south.
- The deep salinity maximum deepens from 201 to 325 m; the confidence intervals still do not overlap.
- Northern Adriatic temperature differences improve markedly, to 88 % of the reported combinations significant.

Two costs of the longer window are stated in Sect. 2.1 rather than left implicit: Argo now supplies 44.3 % of 21st-century southern Adriatic casts against 32.8 % to 2019, and none in the 20th century; and the years 2020–2025 supply 29.4 % of the 2000–2025 months to the Mediterranean basins but only 20.7 % to the Adriatic. Neither affects the deep results.

Two further corrections made during this revision

The summer surface temperature difference is a sampling artefact. The −1.22 °C in the 5 m summer bin of the southern Adriatic reflects the shift of the first sampled depth from the surface to 2.5 m (5.8 m for Argo) across a 0.34 $°C\,m^{-1}$ thermocline, not a cooling; the bin is no longer interpreted (Sect. 3.1).

The region polygons are not those of Manca et al. (2004). Only the labels DJ1 and DJ3 follow that nomenclature; the boundaries follow the coastline and bathymetry. The submitted version attributed the boundaries to Manca et al., which was incorrect. Sect. 2.2 now states this and the polygon files are archived with the code.

Comparison with recent related work

Two studies published since submission bear directly on Sect. 3.7 and are now compared against explicitly.

Amorim et al. (2024), from the EMSO-E2M3A mooring, report the salt-finger regime rising from 27 % to 72 % of observations at 350–550 dbar across the 2016/17 event. Our Argo-based diagnostic over 200–1000 m showed the finger-favourable *extent* unchanged, which appeared to conflict. Recomputing on their layer resolves it: at 350–550 m the finger-favourable fraction rises from 48.6 % to 60.2 % across the record. Both analyses therefore find a significant increase in the same layer across the same transition, and Sect. 4.2 now claims no more than that, and the comparison has been moved out of Results into the Discussion, so Sect. 3.7 reports only our own analysis. We have removed the earlier statement that the two agree in order of magnitude: their rise is 45 percentage points against our 12, and the two percentages are not the same quantity in any case — theirs is a fraction of measurements in time at one fixed position, ours a fraction of levels across profiles spread over the pit, so a mooring resolves an episodic extreme that a basin-wide sample damps. This prompted us to abandon the 200–1000 m aggregate altogether. Section 3.7 and Table 5 now report three layers separately, because averaging them cancels the signal: nothing changes inside the classical 200–400 m core, while below it both extent and vigour rise, most strongly in the deep layer.

Le Meur et al. (2025), from three EMSO moorings over 2012–2022, identify gravity currents reaching the deep South Adriatic in only four years: 2012, 2017, 2018 and 2022. This is now cited in Sect. 4.2 as the observational basis for treating deep ventilation as episodic.

How the amplification ratio is computed

Reviewer 1's comments 3 and 9 turn on this number, so its construction is now stated in full in Sect. 2.4 and its robustness to the surface sampling change is tested in Sect. 3.3, where restricting the surface layer to 10–50, 20–50 and 30–50 m moves the temperature ratio from 0.55 to 0.38, 0.37 and 0.35. The upper-layer temperature deficit is therefore robust and, if anything, understated.

Citation corrections

Rebuilding the reference list surfaced twelve defects in the submitted version, adjudicated against DOIs and volume numbers. Six were in entries the revised manuscript still cites, and are corrected:

1. Raicich — reference list gave 1999; the DOI and J. Mar. Syst. vol. 9 give **1996**, as cited in the text.
2. Cetinić — reference list gave 2023; the DOI gives **2024**, as cited in the text.
3. da Costa et al. — cited as both 2023 and 2024; the DOI gives **2024**.
4. Chiarini et al. — cited as 2022a,b but only one entry present. Resolved.
5. Schroeder et al. (2017), Sci. Rep. 7, 4065 — the reference list gave the first author's initial as S.; it is **K.** (Katrin Schroeder).
6. The depth range attributed to Artegiani et al. (1997) in Sect. 3.4 for the classical single salinity maximum was not the figure that paper gives. Sect. 3.4 now cites Artegiani et al. (1997) for the 200–400 m range of the southern Adriatic salinity maximum, which is the description our detection is tested against.

The remaining six — Orlić et al., Kunkel et al., Tramblay et al., Mamoutos et al., Sannino et al., and the two Buljan and Zore-Armanda entries missing from the list — were all in passages of the Introduction and Discussion that have since been cut, so those references no longer appear in the manuscript and the defects are moot. We record them here rather than silently dropping them.