

Reply to the reviewers' comments on
Estuarine mixing

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We are grateful for the two constructive and in-depth reviews of our manuscript by an anonymous (Reviewer 1) and Robert Hetland (Reviewer 2) and John Huthnance as editor. All comments helped us greatly to improve the presentation of our results.

Please find below our comments explaining how we have reacted to the issues raised by the reviewers. The review text is written in *italics*, and our response is written in **blue**. New manuscript text is set in **bold**. Along with this rebuttal letter and the revised manuscript, we provide a version of the manuscript that shows the changes. Note that some changes are not indicated, e.g., in the abstract, in some equations and the references, since the tool we used for this (`latexdiff`) does not allow for this.

Reviewer 1

This manuscript is a detailed discussion of the use of the Salinity variance method to quantify mixing in estuarine models. This technique was described by Burchard and Rennau (2008) primarily as a method to quantify numerical mixing in models and was exploited by Li et al. (2018) to quantify the spatial and temporal structure of mixing (defined as the explicit destruction of vertical salinity variance and assess the role of straining and advection in the process. The paper does an excellent job describing in somewhat excruciating detail the development and use of this method, it's relationship to the estuarine exchange flow and follows with a discussion of various mixing process in estuarine systems. Overall, the paper will be of great interest to the estuarine community, and the detailed description of the development and use of this method is commendable. Thanks a lot for this general review of our manuscript.

However, I feel like the paper is way too long and this will limit its impact. I must admit that I am somewhat torn by this in that the length of the paper allows for detail that appears on one publication, and this would be useful for the community but if the authors could reduce the length of the paper by 20% or so I think the impact of this review would be increased. I really don't have any specific criticisms regarding the material covered and as far as I can tell the authors have done an excellent job providing an overview of this topic. If the

editor agrees with my assessment that shortening the paper is warranted- I leave it to the authors to decide what to omit in the revision.

We have now substantially shortened the body of the mixing review by moving more parts to the appendix. These are the salinity-variance and salinity-square equations from Sec. 2.2 (as proposed by reviewer 2), as well as the one-dimensional example for the numerical mixing. We are convinced that we need to keep these appendix contents for the readers interested in these details, which cannot be found anywhere else. By doing so, we could reduce the body of the review text from 46 to 42 pages, despite the additions recommended by reviewer 2. We believe that with this, we generated a concise review on estuarine mixing that will be useful for the estuarine research community.

Reviewer 2 (Rob Hetland)

This review of new methods to quantify and understand mixing in estuaries is timely and important, and will be very useful for the community. I have a number of comments that are primarily oriented around changing emphasis. I think the topics covered and structure of the paper are reasonable. I also think the choice to focus on the Elbe estuary is fine as it is representative of an important class of estuaries. Thus, I recommend publication after (somewhat) major revisions to the text, but do not recommend restructuring or any additional analysis. It should also be noted that my recommendations are mainly just opinions that the authors may implement as they choose. As it stands the text is accurate and more or less complete.

Thanks a lot for this critical review of our manuscript.

Major comments:

The theory discussed in this paper is kinematic description based on the distribution of salt in the estuary, but of course the distribution of salt depends on estuarine dynamics. I don't think that the paper needs to change focus to include a long section on dynamics, there are other reviews that cover this, but I do think that this relationship should be discussed early in the paper. Right now, for example, the Simpson number is first introduced about halfway through the manuscript. I would suggest to review the big concepts in estuarine dynamics in section 3, and relate to them throughout where it makes sense.

This is a good point. We have now included a short introduction into the *Basics of estuarine hydrodynamics*, as a new Sec. 3.1. This includes concepts of estuarine circulation and estuarine stratification, both discussed based on the Simpson number. The definition of the Simpson number has therefore been moved up to this new Sec. 3.1.

I think that there are some assumptions built into the analysis of the Elbe and similar estuaries that make this review useful for a particular set of estuaries but not all. An example of this bias is on line 624, where the authors state "fluctuations at the open boundary are typically small." This is not true for 'short' tidal estuaries, as described by Chen et al. (2012). Specifically, while the analysis presented in the paper can be performed on Merrimack, other metrics

are probably better suited for characterizing that small, tidal estuary. For this reason, I suggest being more explicit about this bias in the text, and changing the title to be more specific than the overly broad ‘Estuarine Mixing’. Perhaps ‘Estuarine Mixing in Large Estuaries’ or similar.

It is correct that the Knudsen mixing relation (24) is not accurate for estuaries with strong salinity fluctuations at the mouth of the estuary, such as the Merrimack estuary. The reason is that then the differences between $(s^2)_{\text{in,out}}$ and $(s_{\text{in,out}})^2$ become evident. To make this clear, we have now added after eq. (25): **For the special case of $(s^2)_{\text{in}} = (s_{\text{in}})^2$ and $(s^2)_{\text{out}} = (s_{\text{out}})^2$, (24) is identical to (25). These equalities are only exact when the inflowing and outflowing salinities are constant in time and space during the averaging interval (Burchard et al., 2019). For estuaries with strongly fluctuating salinities at the mouth (such as for the short Merrimack estuary, see Chen et al., 2012) relation (25) has to be used instead of (24) to obtain an accurate estimate for the mixing.** With this, there is also a relation for short estuaries like the Merrimack, such that the *Mixing review* is relevant not only for large estuaries. Furthermore, the universal law of estuarine mixing, (35) and (36), is valid for all estuaries, including those with a high salinity variability at the open boundary. To emphasise this, we have now added in the paragraph after eq. (37): **While the Knudsen mixing law (24) is only valid when salinity fluctuations at the open boundary are limited, the universal law of estuarine mixing, (35) and (36), is exact for all estuaries (without relevant freshwater fluxes across the sea surface).** Due to the generality of these relations, we refrain from changing the title of this *Mixing review* to a more specific one.

I don't like the analogy to mixing in tea, which I find too pedantic, and suggest replacing that with a more mathematically based discussion oriented around Figure 1. The rest of the paper is quite mathematically heavy, so no need to present mixing from an undergraduate perspective. More generally, I think that the discussion of stirring vs mixing needs to be more explicitly related to estuarine circulation. In estuaries, gradients are generated through strain, but estuaries typically do not contain an unresolved submesoscale eddy field which would 'stir' the salinity gradients. Orienting the discussion around the actual unresolved circulation features that cause stirring in estuaries will be important to differentiate estuaries from other oceanic flows.

During conferences, we see it so often that scientists use the term *mixing* without a clear definition. Often they say mixing but mean TKE, dissipation rate, eddy diffusivity, buoyancy flux etc., and often the meaning changes within one presentation or paper, leading to completely wrong statements about mixing. Therefore, we find it essential to have a clear example for what mixing is and how it occurs. To make the tea cup example more estuarine, we replace the tea mixing with mixing of salt water with fresh water. The math is still the same, but the numbers are different. Directly after the idealised lab example, we discuss briefly how stirring occurs, and refer to observational Figs. 17 and 18 for a demonstration how stirring and mixing can occur in an estuary: **In real estuaries, stirring typically occurs due to vertical shear instabilities driven by tidal flow, generating large eddies as shown in Figs. 17 and 18 in Sec. 5.1. Via the classical turbulence downward cascade,**

smaller and smaller eddies are generated such that finally mixing is enhanced at the smallest scales. Since estuaries are typical narrow and friction-dominated, horizontal instabilities on the submesoscale (McWilliams, 2016) play a minor role for stirring in estuaries. An exception would be large fjord-type estuaries with weak tides such as the Baltic Sea (Chrysagi et al., 2021).

Another example of this is in the discussion of numerical mixing. Schlichting et al. (2023, 2024) find much higher fractions of numerical to physical mixing, I suspect because of the character of the flow. The submesoscale field creates strong convergence at the fronts, and this drives numerical mixing. In estuaries, this kind of circulation does not exist, and to the extent there are convergent flows, e.g., at shoals, these are determined by topographic interactions. This is likely also related to the success of numerically diffusive unstructured grid models in estuaries – the large numerical mixing is swamped by the even larger physical mixing.

Yes, although in the presented model results for the Elbe River estuary the numerical mixing is small, it can be much larger in other models. In the beginning of Sec. 5.2.2 (Numerical mixing analysis), we have therefore now added: **Although the numerical mixing in the model of the Elbe River estuary is relatively small (see Figs. 4 and 8), it can be of considerable size in other estuarine models (Ralston et al., 2017; Henell et al., 2023). Therefore, the quantification of numerical mixing is discussed here.** Under the heading of **Measures to reduce numerical mixing**, we have now added: **A further reduction of numerical mixing can be achieved by aligning the horizontal grid with the major tidal flow directions. The present Elbe River estuary model (adopted from Reese et al., 2026) as well as the Weser River estuary model by Rummel et al. (2025) align the curvilinear grid with the dredged navigational channel and not, as typically done, with the lateral shoreline. This reduces numerical mixing because in estuaries lateral salinity gradients are generally much larger than longitudinal ones, such that these specially constructed horizontal coordinates align with those gradients which then do not have to be advected with the along-estuary flow across grid interfaces.** We have also added a reference to Schlichting et al. (2023) who show that the offline analysis can be quite inaccurate.

Finally, I think the discussion of mixing in section 2.2 could be shortened, as this is not necessarily particular to estuaries.

We have now moved the salinity-variance and the salinity-square equations down to a new appendix section.

I would like to see the discussion of TEF expanded. I think this section could use more examples or analogies to make the ideas more clear. TEF is central to modern thinking about estuarine exchange, and I think that a review paper on this topic should lay a solid foundation for people new to the field.

We agree that TEF is essential for modern thinking about estuaries, but since this review focusses on estuarine mixing, we think it would be beyond the scope of this review to include an in-depth review of TEF. Instead, we add some more

clarifying text at the beginning of the Sec. 3.2: **The estuarine exchange flow of water masses defined by salinity, i.e., the net inflow of high salinity ocean waters and the net outflow of low salinity estuarine waters, can be best quantified in terms of time-averaged transports in fixed salinity classes. The resulting Total Exchange Flow (TEF) provides an analysis framework based on salinity coordinates rather than geopotential (z -)coordinates which is consistently linked to the Knudsen theory as well as to estuarine mixing. As shown by several authors (MacCready, 2011; Sutherland et al., 2011; Burchard et al., 2018), the Eulerian (z -coordinate) framework could be mapped back to time-averaged salinities, but the resulting exchange flow profiles would significantly underestimate the exchange flow. Therefore, the TEF framework has developed into a major research tool for analysing estuarine dynamics. For the Baltic Sea, approaches similar to TEF have already been developed earlier (Walin, 1977; Döös et al., 2004). Here, we briefly explain the theoretical framework for TEF and refer to the literature for the details (MacCready, 2011; Burchard et al., 2018). We further cite a number of examples for using TEF in estuarine analysis at the end of. Sec. 3.2: **The TEF analysis framework has been applied for a variety of estuarine studies, such as for tidal estuaries (MacCready, 2011; Chen et al., 2012; Wang et al., 2017; Conroy et al., 2020; Lemagie et al., 2022; Reese et al., 2024), tidal bays (Gräwe et al., 2016; Rayson et al., 2017; Xiong et al., 2021; Lemagie et al., 2022), non-tidal estuaries (Lange et al., 2020; Burchard et al., 2025), inverse estuaries (Lorenz et al., 2020), fjords (Sutherland et al., 2011; Lemagie et al., 2022; MacCready and Geyer, 2024), and regional seas (Döös et al., 2004; Burchard et al., 2018).****

The discussion of the universal law of mixing would benefit from some comparisons to my work on river plumes, based on earlier work by Garvine (1999). Garvine noted that mixing and plume area were inversely correlated, which I later quantified as fresh water mixing through plume isohalines; the universal law is seen in the steady state version of equation 4 of Hetland (2005). I think examples like this will help to connect estuarine mixing to related shelf processes. One place this is already hinted at in the manuscript is the paragraph starting on line 604, where high flows push the mixing out into the Changjiang plume. This might just be seen in the context of the universal mixing law, where the estuary is fresher, and so mixes less (i.e., smaller S at the mouth in $Q_R S^2$, but the total mixing in the estuary/plume system still needs to approach $Q_R S_{\text{ocean}}^2$).

We agree that already Hetland (2005) had derived a formulation equivalent to the universal law of estuarine mixing and acknowledge this now by stating (right after eq. 34): **A relation equivalent to the combination of (33) and (34) had been derived by Hetland (2005), based on earlier work of Garvine (1999), to quantify the turbulent salt transport into river plumes due to entrainment, see the steady-state version of his equation (4).** At the end of Sec. 3.5, we have further added: **This is also in line with the study of Garvine (1999) who showed that increased (background) diffusivity would decrease the area of a river plume (see also the river plume study by Li et al., 2024, who showed that additional**

mixing due to islands in the plume region would decrease the plume area and volume). To link the discussion of the Changjiang River estuary plume mixing to the universal law (close to the old line number 604), we have now added: **In the sense of the universal law of estuarine mixing (32), this can also be formulated as follows: Since the estuary is relatively fresh during high-flow conditions, mixing inside the estuary is small. Therefore strong mixing must occur outside the estuary, i.e. in the river plume, to amount to $\mathbb{M}(S) = S^2 Q_r$, where here S is a salinity separating the estuary - river plume continuum from the adjacent coastal ocean.**

I found the discussion in the paragraph starting on line 614 interesting. It seems that the result of Broatch and MacCready (2022) can be explained by the universal law of mixing, since for the (large) Puget Sound, S at the mouth will not change much, but Q_R can vary by an order of magnitude. Generally, even though S is squared in the mixing term, increasing its importance in mixing, Q_R often has more variability in large estuaries.

Very good point. We have added now: **This can specifically be supported by the universal law (32): Since the salinity at the mouth of the large Puget Sound might not change much, the volume-integrated mixing should be largely proportional to the river discharge.**

Minor suggestions:

In section 3.3 I think I would prefer to see simpler notation. Specifically, I think the $_{\text{dia},z}$ subscripts are cumbersome and should be simplified. Perhaps only u_{dia} needs to be differentiated from the general Eulerian u ; it seems f and j are always diahaline variables.

We had discussed this at length with all coauthors, but in the end agreed to have the notation somewhat consistent with the key paper by Klingbeil and Henell (2023), in order to give interested readers a chance to dive deeper into the theory without much confusion.

Line 511: Delete second ‘many others’.

Done.

Line 534: Perhaps ‘vertical mixing’ is not intended here, but rather vertical turbulent diffusivity? If vertical mixing was intended, rewrite the sentence to be more clear.

Correct. We have now changed **vertical mixing to turbulence**.

Figure 11: The bottom panel is hard to read. Suggest to separate into two panels.

The longitudinal and the lateral straining have now been moved to a new third panel.

Line 567: change ‘not suppressed’ to ‘not completely suppressed’?

Done.

Comments from editor (John Huthnance):

General comment:

From the referee comments you might like to consider whether some of the more-detailed sections might appear in Appendices. Please be sure that, if you do so, connections between Appendices and main text are strong.

We have done this now, see our response to the comments by reviewer 1.

Detailed comments:

Line 55. "higher". I would prefer to avoid "high" and "low" where the quantity is not elevation (temperature is one exception by convention). Here "greater" or "more intense" are alternatives. There may be other places to consider, e.g. figure 15 caption ("less" or "more saline"?), lines 890, 925, 931, 932, 943, 1028 ("finer resolution" or similar).

Corrected.

Line 96. ". . data . . are . ." is preferable.

Corrected.

Line 250. "where triangular brackets denote temporal averaging" (line 297) should be stated here since (16) uses where triangular brackets to denote temporal averaging.

Well spotted, thanks a lot. We moved the half sentence up now until after eq. (16), which has now been changed to (14).

Lines 528-529. I think this sentence would read better if starting "These theories and examples . .". Also "it" → "mixing" in line 529.

Corrected.

Line 788. "corresponding buoyancy production" appears adjacent to "vertical mixing leads to an increase of potential energy" and reads strangely; I guess the sequence may be buoyancy production → stratification → mixing → PE increase → TKE sink.

Changed to **For stable stratification, turbulence leads to mixing as well as negative buoyancy production, which leads to an increase of local potential energy and acts as a further TKE sink term.**

Line 791. Omit 2nd "to"?

Corrected.

Line 940. "make a difference" → "distinguish"

Corrected.

Line 953. ". . reproduction of . ."

Corrected.

Line 969. Omit 1st "of"?

Corrected.

Editorial comments:

1) Please do not use coloured text in your manuscript (equations).

We have now changed the coloured fonts in our equations by an underline.

2) Please remove the section "Contents" (pages 2 and 3).

Done.

3) Your reference list includes a work "submitted". Such works can be cited upon submission if being available to the reviewers. They should not be cited in the final, accepted manuscript, unless published, accepted for publication, or available as preprint with a DOI.

The paper by Mucbe et al. (2025) has been accepted for publication in the meanwhile and is now cited as Mucbe et al. (2026)

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