This manuscript by Ruan et al. uses two simulations of the North Atlantic ocean to assess the sensitivity of carbon export to the representation of mesoscale turbulence, as determined by a change in model resolution from 1/4° to 1/20°. The authors diagnose various contributions to the total flux of carbon across the base of the mixed layer, including advection, small-scale mixing and mixed layer depth variability. They find a remarkably constant ratio (2:1) of mixed-layer to advective export of POC across regions with distinct dynamics.

While it has been well-established in the literature at this point that the advective export of tracers is sensitive to model resolution, there are still open questions that a model-comparison study like this one could shed light on. One potential novel contribution is the explicit inclusion of DOC, whose contribution to the physical injection pump is not well understood even though it is more sensitive to advective processes than POC, whose gravitational sinking term is invariant between the two models. In addition, the authors calculate export fluxes from the time-varying base of the mixed layer, which is more physically meaningful than a fixed depth horizon.

In the manuscript's current form, the results are largely limited to simple diagnostics of fluxes from the model. We feel that there is an opportunity to use the model output to provide a more mechanistic analysis of the physical processes that give rise to the carbon fluxes in these simulations. This is of particular interest because the simulations span a range of physical regimes with quite different stratification, mixed layer depths, mixed layer variability, and mesoscale activity. While a key result of this study is the relative insensitivity of advective export ratio across these regions, the reason for this is not explored. Discussion of the drivers and impacts of DOC export is limited, even though it increases by almost 40% in the eddy-rich model, compared with 10% increase in POC export.

We also have concerns about how the export fluxes in eq (1) are defined, which may affect the main conclusions of the study. As written, mixed-layer export appears insensitive to differences in concentration across the mixed-layer base; other variables need clarification . An explicit explanation of how eq (1) are derived is needed to evaluate whether the fluxes are appropriately defined.

Major comments

- We encourage the authors to provide a more detailed analysis of the dominant processes that contribute to export and how they vary across regimes. For example, wavenumber and/or frequency spectra (or co-spectra) would provide interesting insight into the dominant spatial scales of variability in both vertical velocities and export fluxes in the simulations. Specifically, offering an explanation or hypothesis for why the export ratio remains constant across regions with quite different EKE magnitudes would strengthen the study.

- Because the study's main conclusions are based on the export framework in eq (1) and how the mixed-layer budget is separated into these different contributions, it would benefit from a more careful and detailed explanation of how these terms are derived.

The definition of C_h is unclear. Line 203 states that C_h is the concentration at the base of the mixed layer, but the definition of the left-hand term $h\,\partial_t\,C_h$ (the tracer rate of change in a mixed-layer volume assuming a vertical uniform tracer concentration inside that volume, Lines 204-205), suggests that C_h is also the average mixed-layer concentration. Similarly, what is E_C, and is it a mesoscale eddy term or meant to represent submesoscale eddy terms as well? How is the sedimentation term S_c^{sink} defined?

- The derivation of the mixed-layer export terms also needs further clarification. The derivation of an entrainment term from integrating the tracer evolution equation would normally look like $(C_h - \bar{C})\partial h/\partial t$, but $\bar{C}$ doesn't appear anywhere in this equation. This implies that both entrainment and detrainment flux are both proportional to C_h (that is the average mixed-layer concentration), although we would expect entrainment to depend on the concentration below the mixed layer as well. Overall, the choices made by the authors to arrive at eq (1) may be reasonable, but we encourage them to clearly describe the assumptions and validate them first.

- Additionally, the entrainment term, $-\,C_h\,\partial h/\partial t,$ by definition contributes positively to the mixed layer tracer content when the mixed layer shoals, whereas in other frameworks, e.g. Dall'Olmo et al. (2015), detrainment from shoaling actually contributes an export to the interior by leaving detrained organic carbon-rich water behind. Is this a sign error, or a different characterization of the mixed-layer pump than what has been described previously? If the latter, please provide context.

- The manuscript could do a better job of putting these results in the context of previous observational and process-based studies of the mixed layer pump and the carbon export pump. The introductory material seems to focus on dynamics typically associated with submesoscale motions, but the authors do not provide a clear statement of the dynamics that are adequately captured in the high-resolution 1/20° model. Fluxes related to mesoscale and submesoscale structures will have different characteristics.

- Validation of the model outputs is limited to Figure A1, which compares surface climatological values. In particular, while the physical outputs of ORCA025 and VIKING20X have been validated previously, the BGC outputs from coupling PISCES need additional validation, particularly how they represent vertical structure compared with climatology. The DOC discrepancy with data is explained as a mismatch in what's being resolved/measured, and it's not clear how to identify the semi-labile component from observations, but this does not give confidence in the DOC export estimates overall.

In Fig. 1c, the sign of DOC export is large and positive almost everywhere in the subpolar region, but it contributes negatively in Fig. 2. We encourage the authors to check the sign of the fluxes here.

Minor comments

• Almost all the citations related to the eddy subduction pump are to Omand et al. (2015), Boyd et al. (2019) or Resplandy et al. (2019). It would be nice to put this modeling study in the context of some recent field work that have addressed these topics, such as the APERO, EXPORTS, CALYPSO, etc. projects. Furthermore, there have been a number of important studies that address the relevant spatial scales and mechanisms that dominate the eddy subduction pump, such as Stukel et al. (PNAS, 2017), Freilich et al. (PNAS, 2024), Dever et al. (GBC, 2021), Ruiz et al. (JGR, 2019) and Lang et al. (JGR, 2026), to name a few. Again, it would be nice to put some of these studies in the context of the types of motions resolved by the simulations. .

• Paragraph ending in line 60: This paragraph would benefit from a brief discussion of how the ESP has been inferred from observations previously and how this fits into the model framework (see comment above).

• Line 75: "eddy-rich refinement in the Atlantic that is capable of resolving mesoscale eddies" Suggest discuss the differences between export at the mesoscale vs. submesoscale.

• Paragraph ending on line 91: I appreciated this clear explanation of terminology used in the manuscript!

• Line 110: "unevenly" should be "uneven"

• Line 149: Please include information about surface forcing in the experiments.

• Lines 168-170 - why use three separate clustering definitions for the observations, the ¼ and the 1/20 deg models? Using the same boundaries everywhere would make the comparisons more robust, particularly between the two simulations

• Line 256: "This strong seasonal variability leads to substantial cancellation between periods of influx and efflux, resulting in a relatively small annually averaged mixed-layer export …" This has been shown previously – add a citation to Bol et al. 2017 or Dall'Olmo et al. (2016)?

• Line 273: "1/20°" is written twice – the second time should be "1/4°"

• Line 281: "The increase in advective export generally exceeds that in sedimentation export, suggesting that the advective component of POC export is particularly sensitive to model resolution. This sensitivity likely arises because higher resolution enhances small-scale variability, which can also modify large-scale fluxes" This is not surprising -- add citations to previous work showing that vertical velocities and vertical fluxes are enhanced in high resolution

models?. It would be nice to provide at least some basic analysis of the dominant scales of w_h or lateral export. (Same comment applies to the sentence starting on line 311).

• Paragraph starting on line 315: This section needs to discuss the scales and physical processes that the simulations are resolving. It would be nice for the authors to mechanistically explain why they disagree with the conclusions of Resplandy et al. (2019).

• Line 335: “alongside the gravitational pump, subduction export plays an important role in determining the seasonal timing of total organic carbon export, with important implications for the design of future ocean observing systems.” This has been discussed previously – consider adding one or more citations, e.g. Erickson and Thompson (GBC, 2019); Chen and Schofield (GRL, 2024)

• Paragraph starting on line 349: This paragraph is vague – it would be helpful to discuss the caveats associated with a spatially and temporally evolving mixed layer depth with specific reference to equation (1).

• Line 374: “remove eddy effects” What are remote eddy effects?