Responses to the reviewers for the paper

"Non-local Eddy-Mean Kinetic Energy Transfers in Submesoscale-Permitting Ensemble Simulations"

by Quentin Jamet, Stephanie Leroux, William K. Dewar, Thierry Penduff, Julien Le Sommer, Jean-Marc Molines, and Jonathan Gula.

We thank the editor for handling this revision, and the reviewers for their interest in our study and for the quality of their comments. Our detailed responses follow. Reviewers comments are indicated in **bold**, and our responses are in blue. In the "tracked changes" version of the revised manuscript we have highlighted the significant corrections from the previous version, in response to the reviewers comments, by highlighting them in bold characters.

In addition we have added data and software statements in the Acknowledgments section, as requested by the editor to adress AGU's Data and Software policy.

Detailed responses to reviewer #1:

The present paper analyse the kinetic energy budget from submesoscale permitting simulations (I guess the exact geographical location of the model on is relatively unimportant). An ensemble as opposed to the more usual (though not necessarily "better") time/space averaging is employed, and the kinetic energy budget is analysed with the mean taken be ensemble mean, and eddies being the deviations from the ensemble mean. The key finding here is that there are significant non-local transfers when submesoscales spatial extents are concerned, but less so on the mesoscales spatial extents. The ensemble of high-resolution "truth" constraints on parameterisation efforts, and provides further insights into ocean energetics.

The paper is largely well-written (some of my pedantic comments later), the scientific approach (using ensembles) and methodology (using CDFTOOLS and offline diagnoses with frequent enough outputs) is sound, and results are scientifically interesting, so I recommend the acceptance with minor revisions. The comments I have are really just text and notation edits and/or suggestions.

Specific Comments:

- This one I am going to steal from Stephen Griffies. There are a not-negligible amount of instances of "this", "that" and "it" being used, when one could help the reader by being more explicit about the subject being referred to, as it would reduce the number of times the reader has to jump back and possibilities for ambiguity.

Thank you, we have reviewed the wording in order to reduce these redundancies and clarify the text.

- I am going to pick on the mathematical notation, to do with for example Eq. (4) and the underlined terms in (8) and (9), and the multiple and related subsequent occurrences (e.g. figure captions, in text, other equations). In Eq. (4), one could have
div u K = div (u K)

or

div u K = K div u + u \cdot \nabla K,

and it is not immediately clear which is meant until one goes through the derivation. Also, once the reader remembers we are dealing with evolution of scalars, then we should have in Eq. (8)

u' u_h' = a 2-tensor field (not bothering with distinguishing co/contra-variance)

div (u' u_h') = contraction back to a vector (as above)

\cdot above = scalar

In that regard there really should be some sort of tensor/outer product on the first term. One plausible misinterpretation might be (which is what I did first):

\cdot grad = scalar operator like the material derivative

= multiplying two vectors? Is there a typo and so there is a dot missing?

To me the notation is causing unnecessary ambiguity that is relatively easy to avoid. I would personally have used index notation, though I can see why the authors might not want to do that, so I would suggest adding some brackets as well as the tensor/outer product symbol in to make it absolutely clear the ordering and intent of operation involved.

Thank you for pointing this ambiguity in our notation. Following your recommendations, we have added brackets where $\nabla\cdot$ operator was confusing, and made explicit the definition of Reynolds stresses by adopting a tensor notation.

- Mak et al. (2018) unlike the other cited papers in the relevant sentences do not use mixing length or the EKE (introduction here as well as in conclusion). There is a purported claim that the result from Marshall et al. (2012) (precursor to Mak et al. 2018) is a special case of mixing length arguments, but that is a coincidence, and in my opinion a uncalled for over-claim: The proposed k_gm functional relation comes purely from a mathematical bound of the EP tensor and GM, and uses the total kinetic + potential eddy energy (e.g. Maddison & Marshall, 2013, Journal of Fluid Mechanics). Probably want to modify the text accordingly.

Thank you for this comment. We agree the approach of Mak et al (2018) differs from other cited papers in their formulation. We have made this explicit in the Introduction.

-line 68-69: "or through Eliassen-Palm eddy stress tensor (Marshall et al., 2012; Mak et al., 2018)"

- (Personal opinion: I sometimes get the feeling we the community are still sticking with mixing length as if it were some universal dynamical principle / divine gospel because we have no very good alternatives, even when the assumptions going into the validity of mixing length arguments become extremely suspect. See also my comments later about the time/space averaging vs. ensemble averaging.)

Thank you for sharing this personal opinion. We have further commented on the use of ensemble in the conclusion (cf below).

- Besides the Aluie et al. (2018), there is also the recent Grooms et al. (2021) in

JAMES about spatial filtering that should be cited too (it's not an entirely fair comparison because of timing reasons, but the Grooms et al. tool is somewhat stronger than the Aluie one).

Thank you for referring to this additional method. We have added it in the discussion
- line 120: "or spatial filtering (e.g. Grooms et al., 2021)"

- It is true we don't normally use ensembles as the averaging operation. However, because we don't use ensemble averaging for these kind of analyses that often, and given the authors have mentioned coarse graining, some discussion speculating on the results found here with ones that might arise if coarse graining were used as an averaging operator instead would be called for. If not a comparison, at least some comments as to why such a comparison might be invalid (which is suggested currently in lines 126-129, but could be reiterated in the conclusion section).
I would also say while the authors note a justification for why ensemble average would be preferred scientifically (lines 126-129), the authors can strengthen that even more by highlighting how "traditional" approaches of time and/or space averaging are actually subject to the ergodicity assumption, which is rather questionable, so if anything the ensemble approach is the "right" one to do (but we don't do it because it's hard and/or expensive.)

Thank you for this comment. Performing a robust comparison between ensemble and time or coarse graining / spatial filtering is out of the scope of this paper. It remains however an interesting discussion and we thanks the reviewer for calling for it. We have added the following lines in the conclusion:

- line 654-675: "We have performed our analysis based on ensemble simulations, with a view of inferring dynamical processes that need to be accounted for in submesoscale parametrizations. The ensemble approach differs from other time averaging, coarse graining or spatial filtering methods. Although a comparative analysis between the different approaches is out of the scope of this paper, we want to point out to two potential benefits of ensemble simulations. First, when considering turbulence as the residual from a time averaging, ergodicity of the system is implied, i.e. the time averaging is treated as an ensemble averaging. Althought such assumption might be valid in the case of a steady forcing, its validity is questionnable for non-stationnary systems. Thus, ensemble simulations may help in examining the response of eddy-mean interactions to changes in the forcing, such as what Uchida et al. (2022) have found for the seasonal variation of Eliassen-Palm fluxes in $\frac{1}{12}^{\circ}$, 48-ensemble member ensemble simulations of the North Atlantic basin. Second, coarse graining (Aluie et al., 2018) or spatial filtering (Grooms et al., 2021) approaches are subject to the definition of a length scale cut-off, thus to the size of the '*eddies*'. However, it remains unclear how the non-local energy transfers we have diagnozed here would depend on this parameter. In particular, questions remain on the spectral expression of MEC, EDDYFLX and DIVEF, as well as their respective contributions in fluxing energy up or down scale. We are currently investigating this last point and will report on the results in a dedicated paper (Uchida et al., In Preparation). "

- So the authors do note that non-locality is important, but could the authors comment (probably in section 4 and/or the conclusion) on how much one could attribute that non-locality to some well-defined transport, which would serve as explicit constraints for parameterisation purposes (e.g. depth-integrated mean flow such as in Mak et al., 2018)?

The comment "In this direction, the emerging approach of transport under *location uncertainty* (LU) for the representation of small scale, stochastic dynamics and its effect on the large scale flow (e.g., Mémin, 2014; Resseguier et al., 2017; Chapron et al., 2018) is an attractive alternative to the mixing length approach" was meant to provide some guidance in this direction. We have further discussed this point by adding the following lines (650-653): "Through a stochastic representation of the transport operator, LU indeed has the potential of providing interesting non-local properties, which will be the focus of future work."

Writing things:

Thank you for pointing these typos/wording issues, which we have corrected unless stated below.

- plain language summary: not that plain language from "turbulent closures" in line 43 onwards...

We have worked on making this more 'plain language', thank you.

- line 44: don't need the "for this"

- line 46-47: jargon not yet defined

- line 63 and elsewhere: so to me "rate of change" implies "time" already (while "gradients" I would use for space and others), so in that sense "time" is redundant, but the authors can decide on this one

Thank you for this comment. We have opted in keeping with current use in order to make the time dimension explicit when referring to time derivatives.

- line 101-102: I am somewhat unhappy about the causality here. I would argue it's not that Ian Grooms added a mean flow to the problem, but he added beta that happens to lead to a mean flow, and the two statements are semantically different. Easiest fix would be to have "when a mean flow is PRESENT in the problem (ARISING FROM THE PRESENCE OF the beta effect in his case)"

Thank you, we have corrected accordingly.

- line 117: "for instance IN disentaglING processes"?

- line 157: \phi is undefined, even if it is fairly clear it should be latitude

- line 157, 165: "gradient OPERATOR"

- line 166: "vertical dissipation OF K"

- line 169: don't need the "it leads to"

- Eq. (11): if the authors are using LaTeX, the \underbrace command here to label the terms would make it a bit clearer for the reader that the terms are DIVEF, MEC and EDDYFLX

- line 314 and elsewhere: if in LaTeX, the minus signs need to be under the dollar signs, because it is coming out as a dash, and is noticeable mostly because the math font plus signs is so much bigger than the normal text minus signs (dash signs really)

- line 349-350: slightly weird sentence, try maybe "...ensemble members suggests it is associated with dissipation"

- line 382-384: Clause before subject leading to slightly unnecessary jumping. Consider "Although the spatial organization of IKE is more pronounced..., it somehow follows..."

- line 436: pretty sure "extenT" is meant rather than "extenD"

- line 442: might be my preference, but the action would be "integrated over the volume", while a descriptor for an object would be "volume integrated", and in this case the former is meant

- line 443: if "MEC" is treated as a singular or collective then it should be "...THE MEC exhibits A weaker signal...", otherwise "...MEC exhibit weaker signalS locally..."

- footnote 5: spelling ("consistency") and missing full stop

- line 460: could just have "...MEC, by computing...", also may or may not want a "the" depending on how MEC is to be referred like two comments ago

- line 462: "these" → "the"

- line 505: "is" → "are"

- line 526: for consistency, either "EDDYFLX" without "the", or the other related terms should have a "the" in front of them

- line 578: don't really need to split the sentence, could have "..., suggests that the processes..."

- line 579: "development of SUBMESOSCALE parameterizations"?

Detailed responses to reviewer #2:

Introduction is well written and very instructive on the present-day context of parameterizations of sub-grid-scale processes. Development of equations in section 2.1 is rather lengthy and I struggled to identify which developments where mostly needed to interpret the results exposed in section 3. Hence I recommend to the authors to express literally, while developing the various equations, which terms are calculated to feed into the results section. Results are sound and supported by relevant diagnostics, hence I recommend publication of this study. However, I recommend revising the text slightly, following the list of suggestions below.

Thank you for this general comments. We have added additional informations about the terms in Eq. (8), (9) and (11) that we explicitly compute and discuss in Section 3 and 4.

- lines 202-207: 'The first term on the RHS of (8) is associated with the advection of FKE by the mean flow, and the underlined term is associated with eddy-mean flow interactions. Their respective contribution for the time rate of change of FKE ($\partial_t \widetilde{K}$) will be further evaluated in Section 3.

- lines 215-218: 'The respective contribution of these tree terms for the time rate of change of IKE ($\partial_t \langle K^* \rangle$) will be further evaluated in Section 3.

- line 242: 'A detailed analysis of their spatio-temporal structure is presented in Section 4.'

Specific Comments:

- line 272 : Is the choice of 50% of the surface currents speed in the computation of the wind stress, the result of sensitivity experiments? If yes, then the authors should explain the tuning strategy for this parameter.

This choice comes from Julien Jouanno's recommendations during the production phase of eNATL60 and MEDWEST60. We have further commented on this in the text. Also note that Renault et al.

(2020)'s ocean current feedback parametrization was note implemented in NEMO at the time of MEDWEST60 production.

- lines 288-292: "The tuning is based on Julien Jouanno's recommendations who performed sensitivity tests on modeled EKE levels with (i.e. 100%) and without (0%) ocean current feedbacks in wind stress formulation, and found 50% as a good compromise to reproduce the level of EKE observed by satellite altimetry."

-line 294 : As the simulations last only 4 months, and cover a region where seasonality is substantial, some discussion on the generalization of the results on longer time scales, is necessary.

Thank you for this comment. Indeed, we expect seasonal variations to imprint into eddy-mean flow kinetic energy transfers between summer and winter time period. We have added a comment on this.

- lines 433-439: "The 120 days of simulation cover the period February, 6^{th} to June, 5^{th}, thus a weakened submesoscale activity associated with spring time is observed toward the end of the simulation as compared to its begining. It is thus likely such a seasonal cycle will imprint onto eddy-mean flow kinetic energy transfers, a signature observed for instance by Uchida et al. (2022). The relatively short time duration of MEDWEST60 ensemble does however not allow us to quantify such seasonality. "

- line 297 : How do eddy-mean interactions scale with amplitude of initial perturbations ?

Micro initial conditions as those used in MEDWEST60-ENS-CI-GSL19 are meant to introduce infinitely small perturbations to trigger the growth of an ensemble spread in response to non-linearities of the system. Thus, there is no scaling between the amplitude of eddy-mean flow interactions and initial conditions.

- Figure 2 : it could be appropriate to change color of the green box as it is confusing with the color of lines

Thank you, we have corrected this.

- line 420 : to my view, the residual includes the uncertainty in the computation of the various terms. If this is right, can you please add a comment on this ?

Uncertainties in the computation of the several terms are indeed part of the residual. However, given their amplitude ($\mathcal{O}(10^{-2} - 10^{-3})$, cf Table A1), we argue the residual mainly reflects the commulative effects of the termes we have not computed explicitly, namely pressure work and dissipation. We have clarified this in the text.

- line 456-459: "The contribution from other processes, such as pressure work, surface forcing and viscous effects, as well as small uncertainties associated with our offline estimates (cf Appendix A), are shown in green as a residual."

- Figure 4 : Offline estimates of viscous effects is very challenging, as noted by the authors. This should be stressed explicitly while interpreting Figure 4, bottom right panel.

Thank you for this comment. We have stressed this explicitly in the caption of Figure 4, as well as in the text (cf previous comment).

- line 558 : typo "of the"

Thank you, we have corrected.

- lines 577 and 616 : If the "next generation climate models" refers to CMIP7, then surely the majority of models will NOT have a resolution as high as 1/12 in the ocean

! Please rephrase to better insert the results of this study in the context of climate modelling. As in reality, for the present-day climate models of resolution 1 deg, the locality assumption of energy transfers seems to be valid, still, which is good news for climate modellers !!!

Thank you for this comment. We have rephrased to better emphasize the implication of our study for climate modelling.

- lines 602-605: "However, correlations lower than -0.5 are found for grid size of about $\frac{1}{2}^{\circ}$ and finer, suggesting non-local dynamics would become leading order contribution as soon as mesoscale eddies are (even partially) resolved. "

- lines 643-647: "In particular, accounting for such dynamics in eddy-permitting ocean models, such as those that will equipe the next generation climate model, could lead to significant improvements given non-locality has been found to be leading order contribution for scales as large as $\frac{1}{2}^{\circ}$"

- Figure 10 : insert makes absolutely no sense to me, sorry !

The insert displays the location of the 4 3^6x3^6 (729x729) grid points square boxes used to compute the correlation, each of those covering a slightly different part of the full (883x803 grid points) domain. Coarsening is computed within each of these boxes (color lines) and averaged together (black line) to gain consistency. We have reformulated the caption of Fig. 10.

- line 598-600 : Consider rephrasing this statement, as I don't understand, here, what does DIVEF leads upon.

Thank you, we have rephrased accordingly to better emphasize the contribution of DIVEF in non-local interactions

- lines 625-628: "We have pointed out that non-local transfers are driven by turbulent fluxes of eddy-mean cross energy term, which are capture by the DIVergence of Eddy Fluxes (DIVEF, $\nabla \cdot \langle \mathbf{u}'(\langle \mathbf{u}_h \rangle \cdot \mathbf{u}'_h) \rangle$). "

- line 742-743 : On the use of a different advection scheme than the one employed by the model. Where is the implicit dissipation of the actual advection scheme employed by the model, factored in, in the offline calculations ?

The implicit dissipation of the advection scheme has been included in the offline recomputation of model simulation as well as for validation. However, when working with eddy-mean (ensemble) decomposition, implicit dissipative esimates are non-trivial to compute and introduce ambiguities in the interpretation. We have thus opted for not including them in our estimates, but rather interpret them as part of the residual. This is mentionned in a footnote on page 10.