

Interactive comment on “The role of vertical shear on the horizontal oceanic dispersion” by A. S. Lanotte et al.

The paper’s Introduction provided a nice motivation for the research. It correctly noted that the role of submesoscale and unresolved scale motions in general circulation models (GCMs) is now of considerable interest in a variety of areas. Regrettably the balance of the paper does not fulfill expectations. Specific goals or questions the various simulations were designed to address were incompletely explained. The conclusions merely restated a few generally accepted beliefs and gave no indication as to how the study advanced our understanding of small-scale motions. The writing style contained an irritating number of unrelated factoids. Finally a couple of relevant references were missing. Detailed comments follow.

Introduction

- The first sentence of 2nd paragraph, PG 2974, could be the first sentence of the paper. The second sentence of this paragraph seems completely out of place. I thought the citations to Celani and Ikawa were distracting. The basic point was already made at the end of the 1st paragraph.
- Paragraph starting on line 30, pg 2075 refers to Ollitrault et al. The most recent reference on pair separation is the exhaustive analysis of approximately 300 drifters performed by Poje et al (Proc. Nat. Acad. Sci., 111, 35, 12693-12698, 2014). It is curious the authors did not cite this paper and compare their analyses to that presented in the Poje et al paper.
- The purpose of the paragraph starting on line 6, pg 2076 is not clear. The point that turbulence is not the only mechanism leading to ‘super-diffusive behaviour’ is well established in the meteorological literature. It would be more helpful if the authors provided some example mechanisms relative to their goals rather than trot out a stale dimensional analysis.
- Same page paragraphs starting on lines 15 and 23. Unfortunately in the literature the term “vertical shear” has several meanings. In the first paragraph the authors probably mean the z derivative of the horizontal velocity. But introducing 3D velocities from ADCPs in second paragraph confounds this. A reference would be helpful on how depth averaged velocities from ADCPs are misleading might clarify the point the authors are trying to make. A bigger issue is the generally neglected role of vertical velocities on the spatial scales the authors are looking at. The authors would have performed a real service if they provided a discussion that clarifies various usages and potential importance of vertical velocity.
- Overall the authors don’t make clear that their analysis does not address small or submesoscale processes, only those unresolved by GCMs.

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- Line 10 page 2078 the authors state that later they discuss different recipes to model small-scale motions. I couldn’t find where they actually did this. They merely introduce their KLM but do not connect it to the cited references.
- The author should provide some physical background for the setting of their KLM rather than merely citing previous usages. It appears to be a special case,

but augmented with time periodicity, of one used by Sulman et al (Physica D, 258, 77-92, 2013). Some comparison with their analysis might be appropriate.

- The discussion of the KLM is confusing. On pg 2081 line 1 the potentials Φ_1 and Φ_2 specified as “streamfunctions”, which they clearly cannot be unless they equal each other. An exponential damping term was added in an ad hoc manner below the mixed layer. This was not included in the original definitions equations 3 and 4.
- Equation 5 defines an A_n . Presumably this refers to an adjustable A given by equations 4 and 5, but this is not explained.
- Same equation defines the perturbation frequency. Since the study is focused on motions unresolved by GCMs. I would expect the frequencies to be inertial to super-inertial, as such phenomena are well known to be missing from these models. Not enough information is given to assess this. Regardless the authors should provide some physical justification why the scales given here are relevant to their goals.
- Page 2081, line 11 introduces the numerical experiments. I could not make a clear connection between the goals of these experiments and what questions the authors wanted to address.
- Page 2082, series II and III. By 2-D KLM do the authors mean w in equation 3 is set to 0? Sulman et al explored the role of vertical velocity extensively so perhaps some connection with that work would be appropriate.
- Page 2082 section 2.2 The FSLE was used by Poje et al. A comparison of their figure 2 with figures 5 and 6 of that paper should be made.

Conclusions. I thought this section was uninformative. The first sentence could be combined with the last paragraph as a summary of the findings. The paper doesn't really touch on the other items mentioned. A well-constructed conclusion would have revisited specific questions raised in the Introduction. This would give readers a chance to take away with some specific messages. As it stands the Conclusions simply restate material I believe most readers already know or suspect.

I did not get much out of the paper that would warrant publication. Perhaps a reworked version that puts the results in some perspective might work. There were a number of syntax lapses and misspelled words but it isn't productive to go through these here.