Response to Reviewer #2

We thank the Reviewer for her/his useful and focused remarks on our manuscript.
In view of them, we have rewritten the paper to a large extent, and we have made additional efforts to make our results clear and of broad impact.
For the sake of clarity, new parts are written in color red in the revised version.

Here below, we examine all points raised by the Reviewer and highlight the changes made in the manuscript.

Reviewers criticisms are here marked in brown, bold face. Our answers in black.
The changes made in the manuscript in red.

Rev: 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.

The first sentences of the paper have been modified partly following the Reviewer suggestion. These now read as follows:
"The role of small-scale motion in geophysical flows is receiving a renewed attention, concerning the hydrodynamical modelling, as well as in relation to the biological consequences of specific phe- nomena. Tracer dispersion in the ocean (Davis, 1983) has an impact on different environmental, chemical, biological and technological problems. Mean currents mostly contribute to the large-scale transport, while small-scale motions tend to spread concentration fields or, equivalently, Lagrangian trajectories of passive or active tracers. Very little is known about the way turbulence and diffusion -in addition to other physical mechanisms-, model marine habitat and promote or impede the life of certain organisms (Ikawa et al., 1998)."

Rev: 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 work of Poje et al., is now cited in the manuscript, both in the Introduction and in the sub-section where the Finite-Scale Lyapunov exponent diagnostic for Lagrangian dispersion is discussed.
In particular, the following text has been added:
Introduction: "More recently, Poje et al. (2014) performed a Lagrangian measurements in the Gulf of Mexico, the GLAD experiment, deploying an unprecedented numbers of CODE drifters. In particular, they quantified pair dispersion rates in agreement with Richardson law. Also, they pointed out that the submesoscale dispersion rates when based on ocean model or altimetric velocities are largely underestimated with respect to the observed ones."
Sec. 3.2 : "Finally, it is worth recalling that, as shown in Lacorata et al. (2014), the FSLE measured for the Mediterranean surface drifters previously discussed follows the Richardson diffusion behaviour $\lambda(r) \propto r{-}2/3$ for $r \in [10,100]$ km. This is consistent with the observed dispersion rates in the GLAD experiment, which spans however a much wider range of scales (Poje et al., 2014)."

Rev: 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.

Since the scaling properties of diffusive behaviours and their origin are not the focus of this work, in the revised version we have removed such paragraph and the one following it.

Rev: 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.

What is meant by vertical shear, that is the vertical gradients of horizontal velocity components, is now clearly stated in the Introduction, where we can read:

"In this paper, we focus on the role of vertical shear as important mechanism promoting the horizontal diffusion in the ocean. By vertical shear, we mean the vertical variation of the velocity horizontal components. The approach here considered consists in combining observative and model data to assess the effect of vertical shear for the tracer horizontal relative dispersion. Observative data come from Acoustic Doppler Current Profilers (ADCP), deployed in the South Mediterranean. Numerical data come from the Mediterranean sea Forecasting System model, and are supplemented with the use of deterministic kinematic models (Palatella et al., 2014; Lacorata et al., 2014), to parameterise poorly resolved mesoscale motions, or unresolved processes in GCMs."

Rev: 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.

In the paper, we mention that on the basis of estimates inferred from the mean flow and *not* from the fluctuating velocities, vertical shear is expected to be much less important than horizontal shear for the oceanic horizontal diffusion (LaCasce and Bower 2000).

As for the vertical velocity, its discussion goes beyond the focus of our work since we do not have direct measurements of vertical velocities available. Our work is motivated by observations and their comparison with model performances.

Rev: Overall the authors don't make clear that their analysis does not address small or submesoscale processes, only those unresolved by GCMs.

The Reviewer is right that we do not consider sub-mesoscale processes and that our focus is on motions unresolved in GCMs.

In the revised version the following paragraph has been added in the Introduction:

"Numerical data come from the Mediterranean sea Forecasting System model, and are supplemented with the use of deterministic kinematic models (Palatella et al., 2014; Lacorata et al., 2014), to parameterise poorly resolved mesoscale motions, or unresolved processes in GCMs. The Kinematic Lagrangian Model (KLM) here adopted can be two dimensional, to better account for the horizontal dispersion due to mescoscale eddies, or three dimensional, to simulate vertical turbulent-like motions in the ocean mixed layer. Both dynamics are often underestimated in General Circulation Models (GCM). Even if our primary interest is in the former situation, we will discuss both."

Rev: 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.

We have changed this part of the manuscript. In the Introduction we now mention different works dealing with the modelling of unresolved ocean processes in GCMs. Also, our goal is not to validate one model with respect to another, but just to use one – and we choose the one we are more familiar to-, to address the problem of verical shear effects on horizontal dispersion.

In the revised version, the following paragraph contains new references:

"Indeed, when dealing with basin scale models, not only the mixed layer dynamics is often missing, but also the velocity field features from sub- to meso-scales are poorly resolved both temporally and spatially. At this regard, various techniques (Griffa, 1996; Berloff and McWilliams, 2002; Haza et al., 2007, 2012) have been developed to model the sub-mesoscale or unresolved velocity components which, nonetheless, play an important role for tracer dispersion."

***Rev: 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*.**

In the revised version we added all details that are crucial for the understanding of the present work, trying to avoid repetitions with previous works. In particular, the 3D KLM model equation (eq. 7) and parameters (eq. 8) are now explained. The 2D KLM model equation (eq. 9) and parameters (eq. 10) are also explained. Since it is too long to paste the KLMs description here, we directly refer to sec. 3.1 of the revised manuscript.

The work of Sulman et al. is now cited in the paper, but we stress that its nature is completely different from that of our work. There toy models for ocean dynamics– such as the (Lagrangian) chaotic ABC or quadrupole flows – are discussed, mostly in terms of the Finite-Time Lyapunov exponent. In this respect, the literature is huge and commenting on it goes beyond the goals of our work.

In the revised version, we added the following paragraph in the new Sec. 3.2

"Furthermore, Finite-time Lyapunov Exponent (FTLE) is also used to detect Lagrangian coherent structures in ocean dynamics applications (Haller, 2000; Sulman et al., 2013). A discussion of the use of scale-dependent indicators in Lagrangian dispersion problems can be found in Berti et al. (2011), while a direct comparison of FSLE and FTLE for the identification of transport barriers can be found in Boffetta et al. (2001)."

Rev: The discussion of the KLM is confusing. On pg 2081 line 1 the potentials Φ1 and Φ2 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.

The 3D KLM model is now presented and the choice of parameters is motivated. Please refer to Sec. 3.1

Rev: 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.

In the revised version, we clearly distinguish the 3D KLM, which is meant to repriduce mixed layer turbulent like mixing, from the 2D KLM which is meant to better account for poorly resolved mesoscale processes. Their appropriate ranges of scales are better introduced and justified. Moreover, it is explicitly mentioned that the 2D KLM is not equal to the 3D KLM with zero vertical velocity. Please refer to the equations in Sec. 3.1.

Rev: 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.

The goals of the numerical experiments are now clearly stated.

In the revised version at the beginning of Section 3.1 it now reads as follows:

"We discuss different sets of numerical simulations based on the velocity configurations of the MFS model, also supplemented by the use of the kinematic model to described poorly resolved motions. Kinematic models can be adapted to the different dispersion regimes, namely exponential separa- tion, turbulent dispersion, and standard diffusion. Their implementation hence depends on the spe- cific dynamics and specific range of scales that one wants to describe. Here, we compute transport properties by introducing statistical Lagrangian motions for the mixed layer motions (3D KLM), and separately for the poorly mesoscale motions (2D KLM). By doing so, we demonstrate that i) small-scale motions enabling tracer pairs to explore the whole mixed layer do not modify MFS hor- izontal dispersion properties, in reason of the anomalous persistence of vertical velocity gradients in the MFS model; ii) differently, the horizontal relative separation resulting from the introduction of the 2D KLM is fast enough to encompass the anomalous shear effect produced by the MFS solution."

Rev: Page 2082, series II and III. By 2-D KLM do the authors mean w in equation 3 isset to 0? Sulman et al explored the role of vertical velocity extensively so perhaps some connection with that work would be appropriate.

This was explicitly mentioned in the previous version. In the revised version, we define the 2D KLM and clarify that the 2D KLM is not equal to the 3D KLM with zero vertical velocity. As for the work of Sulman et al, previous comments hold.

Rev: 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.

As mentioned before, in Sec. 3.2 we now comment as follows:

"Finally, it is worth recalling that, as shown in Lacorata et al. (2014), the FSLE measured for the Mediterranean surface drifters previously discussed follows the Richardson diffusion behaviour $\lambda(r) \propto r{-}2/3$ for $r \in [10 : 100]$ km: this is consistent with the observed dispersion rates in the GLAD experiment, which spans however a much wider range of scales (Poje et al., 2014)."

We think that a direct comparison is not possible since, even if the dispersion rate has the same scaling of the FSLE, its definition is different. Prefactors, either constant or with possible sub-leading scale dependencies, are out of control.

A short list of the main changes is the following:

- Title has been changed.
- Introduction: some general paragraphs have been removed, while more specific comments and references have been added to make it more focused;
- Section 2: it contains the analysis of the ADCP data, and the comparison with MFS estimates;
- Section 3: it contains the numerical simulation results. The 3D KLM model equation (eq. 7) and parameters (eq. 8) are explained. The 2D KLM model equation (eq. 9) and parameters (eq. 10) are explained. The FSLE discussion in sub-section 3.2 has been expanded;
- Conclusions: some summarizing paragraphs have been removed to avoid repetition. A paragraph containing some perspective for this work has been added.
- References: reference list has changed to make it more focused on the topic of the paper.