Response to Reviewer #1

We do thank the Reviewer for carefully considering our work, and for her/his interesting remarks that contributed to improve the manuscript. After consideration of the comments contained in the report, we have largely amended the paper and better presented our results.
For the sake of clarity, new parts are written in color red in the revised version.

Here below, we reply to the Reviewer's comments point by point, better discussing those issues that may not look very clear. We also list what we changed in the paper.

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

Rev: The authors use the FSLE metrics computed from surface drifters as benchmark. More details on the drifter data set should be given. In particular, are they CODE sampling the upper 1 meter, or are they drogued at 15 m?
The drifters are drogued at 15 m. In the revised version, we added details about the drifter data set.
In the revised version, we added the following sentence:
"Drifter data belong the Mediterranean Sea-In-Situ Near Real Time Observations (database INSITU_MED_NRT_OBSERVATIONS_013_035 available on http://marine.copernicus.eu/). These are surface buoys, drogued at a nominal depth of 15 m."

Rev: The authors compare the drifter results with results from MFS trajectories at the surface and they find a significant difference, since the model results show a plateau after approximately 30-40 km while drifter results keep increasing. This is actually a known result, as discussed in details in a number of papers (e.g. Haza et al., 2012), and it is due to the lack of resolution of the model. The authors mention this as a possible reason, but I think they should expand and introduce the references.
We agree with the Reviewer. In the revised version, we acknowledge that the reason for the observed discrepancy is associated to both the lack of spatial and temporal resolutions of the numerical model.
In the revised version, we added the following sentence:
"As it has been previously observed, this discrepancy is due to both the coarse spatial resolution and the time averaging of any mesoscale model (see e.g. Haza et al. (2012) and references therein)."

Rev: More interesting is the discussion on the effects of vertical shear, even though there is a conceptual problem in the way it is presented in the paper. The authors use the same FSLE metric from surface drifters as benchmark, but surface drifters (especially if CODE!!!) do not feel the effects of vertical shear on scales of approx. 40 m as considered in the model and even if they would, it would be a vertically averaged effect, very different from the buoyant particle case of the model. As a consequence, in my opinion this comparison is seriously flawed.
This is actually a delicate point of our work that we will try to make clearer. We do not claim that surface drifters feel the effects of vertical shear, since obviously they cannot. The reason why drifter dispersion is important in this context is the following. When simulating **horizontal** tracer dispersion within an ocean model, one has to decide if

the tracer is fully 3D (e.g., as chlorophyll scalar field in the mixed layer), or if it is constrained to a 2D surface (e.g. as a surface drifter).

Numerical simulations – in the absence of any sub-grid scale model- show a significant difference between the two cases, even at quite small vertical separations: for the MFS surface drifter case, a flat FSLE ($\lambda(r) \sim r^0$.) is observed for separations up to the mesoscale range, indicating a constant exponential rate of trajectory separation; for the MFS Serie I case (a 3D MFS), a FSLE increasing at any given small separation ($\lambda(r) \sim r^{-a}$).

We claim that this behavior is an artifact of the finite resolution of the ocean model, i.e. due to an anomalous temporal persistence of the velocity vertical gradients, and we support this hypothesis by comparison with observational velocity profile data. We also show that such "anomalous shear effect" events are triggered as soon as an elementary sub-grid-scale vertical mixing model is added to the ocean model in order to simulate 3D motions typical of the mixed layer.

Now, the question arises on the proper small-scale ocean model velocity field able to simulate horizontal dispersion of a tracer **having 3D structure** down to, say, the bottom of the mixing layer. The solution to this problem, we believe, is very difficult to find, at least at a kinematic parameterization level.

Finally, another important aspect has yet to be considered. The MFS ocean model underestimates mesoscale turbulent dispersion. The primary correction, in our opinion, is to restore mesoscale horizontal dispersion by comparison with drifter data.

Clearly, mesoscale eddies are not pure 2D structures, but have a certain vertical development in the mixed layer. This implies that mesoscale turbulent dispersion, e.g. the Richardson's regime, is not a property of the surface layer only, but belongs to a whole vertical range of ocean layers. If one applies such "primary correction" for mesoscale eddies, she/he realizes that the effect is to have an efficient dispersion that **covers** the one due to mean vertical shear.

So for the ocean situation studied in our work, restoring horizontal mesoscale dispersion, in agreement with experimental drifter data, automatically eliminates the problem of the anomalous shear effects. We stress that this is what occurs in the case of the Mediterranean Sea dispersion analysed in the present work: but it might not be true in general, and the problem of realistically modelling Lagrangian transport could require different solutions.

Since this is an important point in our work, in the revised version we added the following paragraphs to make this clear from the very beginning.

In particular:

At the beginning of the new Section 3.1

"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 separation, turbulent dispersion, and standard diffusion. Their implementation hence depends on the specific 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 horizontal 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."

At the end of the new Section 3.2

In Series I, the effect of the vertical shear onto the horizontal dispersion comes from the MFS model only. The associated FSLE curves clealry indicates that vertical shear is able to promote horizontal dispersion. Neutrally buoyant tracers moving at different depths experience velocity differences: as a result they start to separate already at very small scales. In Series II, the 3D KLM terms are switched on, and particles vertically explore the whole mixed layer. The obtained FSLE curve is very similar to that of Series I, and in particular it results to be slightly below this last. This finding is somehow surprising since, thanks to the introduction of small-scale turbulent-like motions, tracer pairs can explore the whole mixed layer. However the fluctuations of the 3D KLM do not substantially modify the horizontal pair dispersion, and actually they make it slightly slower in the present case. This implies that the dominant effect is the one associated to the vertical shear.

In Series III, both the 3D and the 2D KLM are switched on. The resulting FSLE is larger than that of series II at any scale. This means that the most important dynamical correction to the MFS model is that associated to the 2D KLM. Indeed, the dispersion effect induced by the mesoscale eddies inserted in the 2D KLM *covers* any other horizontal dispersion effects, including the one associated to the anomalous persistence of vertical gradients in the MFS model.

We can summarise the results of the numerical simulations as follows. By comparing mesoscale dipersion of the bare MFS model with Mediterranean drifter data, one sees that real drifter pair dis- persion follows a turbulent-like type of behavior, whereas model trajectories separate more slowly and at a nearly constant rate. Adding vertical mixing to the ocean model, e.g. in the form of the 3D kinematic model, may trigger a type of shear dispersion which is affected by an anomalous persistence of the vertical velocity gradients, as discussed in Sec. 2. Since we estimate that the anomalous persistence is an artifact of the poor temporal resolution of MFS, the adoption of the 3D KLM only does not seem an appropriate choice. On the other hand, adding a two dimensional kinematic model, one finds that the anomalous shear dispersion effects become practically negligible, being hidden by the more energetic dispersion processes occurring at the mesoscales. Clearly, mesoscale eddies are not pure 2D structures, but they have a certain vertical development in the mixed layer. This implies that mesoscale turbulent dispersion is not a property of the surface layer only, but belongs to a whole vertical range of ocean layers. By adding the 2D KLM for mesoscale eddies, one realises that the effect is to have an efficient dis- persion that covers the one due to mean vertical shear. Finally, it is worth recalling that, as discussed in Lacorata et al. (2014), the FSLE measured for sur- face drifters 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, 2014)”

Also in the conclusion, we added the following paragraph:

“Now, the question arises on the proper small-scale ocean model velocity field able to simulate horizontal dispersion of a tracer having 3D structure in the mixed layer and below. The solution to this problem is very difficult, mainly because experimental data of 3D tracer dispersion are not easily available. Different modelling solutions can be adopted to account for different problems, depending whether mesoscales, sub-mesoscales or small-scales are the relevant range of scales in the dispersion problem. Even the poor man’s procedure of fitting numerical simulations to observations would require an ad hoc mixed layer modelling which is not straightforward to implement.”

Rev: What is interesting is the comparison between the model results, i.e. the MFS surface, the MFS at 2 levels and the case with KLM, that show the effects of shear for a model tracer distributed in the first 50 m. Results indicate that adding a Lagrangian Kinematic Model that parametrizes the action of 3d turbulence does not increase the effects of shear (it actually tends to reduce them in some cases…). The author rational for this, if I understand correctly, is that model vertical shear is unrealistically high because correlation time scales are too long compared with data. This point should be expanded and better explained.

The Referee is right: adding a 3D Kinematic model does not modify the measured effect of shear in comparison with the bare MFS model. This happens because in MFS vertical velocity gradients are anomalously persistent, and the chaotic mixing introduced by the small-scale 3D KLM is sub-dominant with respect of the shear effect of the MFS model. However, the anomalous persistence is no longer a problem when a 2D KLM for mesoscale motions is added. Now it is the latter which gives the dominant contribution, as mentioned above.

In the revised version, we try to make this very important point clearer (read above paragraphs).

Rev: Overall, the paper needs in my opinion some restructuring. My suggestion is the following:- a) The first step should be showing ADCP data to characterize vertical shear in 2 Mediterranean Sea sites. b) ADCP results are compared to MFS results, indicating that correlation times are too long in the model c) The 3d KLM is introduced to parametrize the effects of turbulence and introduce short time scales d) the effects of shear are studied comparing model results: first surface MFS, then cseries I and II. e) I would remove the part on drifter comparison, because I am not sure it adds anything. It can be mentioned as previous results.

We have reshaped the presentation of results following the Reviewer suggestion. Now the ADCP data and their comparison with MFS estimates are presented in Section 2. Numerical results of Lagrangian dispersion are presented in Section 3, where the 3D and 2D KLM model formulations are now discussed in detail and the comparison of different series results is expanded. We kept however the drifter data results, since in our opinion and as explained above, these are relevant for the aims of the paper.

Rev: More in details: where did they get the KLM 3d results?

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.

Rev: The discussion in the Introduction should be more focused and the specific scales of interest should be mentioned.

In the revised manuscript, in the Introduction, we added the following paragraphs:

"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.

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.

The paper is organized as follows. In Sec. 2, we compare in-situ observations of vertical velocity gradients with measures obtained from MFS. The comparison highlights that velocity gradients correlation times derived from MFS are considerably larger than the observed ones: such anomalous temporal persistence of the vertical shear is responsible of an enhanced relative dispersion, which is possibly an artifact of the low temporal resolution of the model. In Sec. 3, we discuss the relative dispersion properties of neutrally buoyant tracers by means of numerical simulations. We show that, by a suitable implementation of the kinematic model, the anomalous shear effect can be overcome. Section 4 contains the final remarks and perspectives."

Rev: The choice of the parameters for the 3D KLM should be discussed and motivated.

In the revised paper, it is explicitly described the 3D KLM and its motivation, namely to reproduce the mixed-layer turbulent like dynamics. In particular, one now reads:

"The small spatial scales l_i and their associated fast time-scales $t_i \sim l_i / A$ are chosen to reproduce, on average, the dynamical properties within the mixed layer"

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.

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 specific 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 horizontal 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.