

***Interactive comment on* “Numerical simulation and decomposition of kinetic energies in the Central Mediterranean Sea: insight on mesoscale circulation and energy conversion” by R. Sorgente et al.**

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Reply to anonymous Referees

The authors thank the two anonymous Referees for their detailed and constructive comments that contributed to a substantial improvement of the paper. The remarks are answered below. In the sections below replied to each general and specific comment of the two Referees:

Reviewer #1 General comments:

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Re: We appreciated the general comments and suggestions given by the reviewer. But on the indication of the area of work, we think that the suggested “Sicily Channel” limits the area too much as we also describe the Sardinia Channel, the south Tyrrhenian Sea and the western Ionian Sea. As geographically a Central Mediterranean Sea does not exist (even if used by some Authors), we have changed in the title from “Central Mediterranean Sea” to “Central Mediterranean” and in the paper to Central Mediterranean region, This indication has been previously used recently Ciappa (2009), but also in Astraldi et al. (1999, 2002) or other authors like in Vetrano et al. (2004) – please see the references below.

Astraldi, M., Conversano, F., Civitarese, G., Gasparini, G.P., Ribera d’Alcalà, M., Vetrano, A., 2002. Water mass properties and chemical signatures in the central Mediterranean region. *J. Marine Syst.* 33–34, 155–177. Astraldi M. G.P. Gasparini, R. Molcard, S. Sparnocchia, Vetrano A. (1999). Hydrographic properties and water exchanges in the Central Mediterranean Region. In: *Geophysical Research Abstracts, European Geophysical Society. The Hague, NL, vol. Vol 1, no 2, p. 399* Vetrano A., G.P. Gasparini, R. Molcard, M. Astraldi (2004). Water flux estimates in the central Mediterranean Sea from an inverse box model. *Journal Of Geophysical Research. Earth Surface*, vol. 109, ISSN: 0148-0227, doi: 10.1029/2003JC002011

Specific comments:

P9, line 0-22: You have to draw the known features Adventure Bank Vortex, MCC, ISV, MRV, etc in Figure 2. You may also add the new features you find in your model like the Pantelleria Vortex (PV introduced in section 4.2.2) in dotted circles for example. It will be nice to distinguish on this figure the cyclonic and anti-cyclonic features with an arrow for example.

Re: the figure has been redrawn as suggested by the Referee. Furthermore permanent, summer and winter features have been distinguished with different colors.

P12, line 11: I think it is S and not S\* in the sentence: ‘. . .R is the river runoff and S\*

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is the surface model salinity. In our. . .

Re: changed accordingly

You can remove R if it is zero in the formulae. Describe what is alpha, H, Delta in C\_2?  
Give the units of these terms. What is Med6?

Re: the used formula comes from Legates and Willmott (1990). The Referee is right that we consider  $R = 0$ , but we would prefer to let R in the formula. If deleted, we should anyway explain why R is not considered. The related sentence has been changed as follows: “The last term of Eq.(3) is the salinity flux correction and accounts for the imperfect knowledge of E-P (P especially).  $S^*$  is the monthly mean sea climatology surface salinity from Med6 dataset, that is based on the MEDATLAS dataset using the MODB (Mediterranean Oceanic Data Base) analyses techniques (Brasseur et al., 1996).  $\Delta s(1)H$  is the thickness of the surface layer, where H is the bottom depth and alpha the relaxation time. The value C2 is equal to 0.7 m/day.” Added the following reference in the References section: “Brasseur, P., Beckers, J.M., Brankart, J.M. and Schoenauen, R.: Seasonal temperature and salinity fields in the Mediterranean Sea: Climatological analyses of an historical data set, Deep-Sea Research, 43, 2, 159–192, 1996”

P13, top: Is the volume of the model conserved? Give some details on the boundary conditions (radiation, prescribed with the coarser model results?)

Re: yes, the model volume is conserved. Since the coarse resolution model is a rigid model, while our model is a free surface (sigma-layer), the barotropic velocity has been adjusted to ensure the conservation of volume. More details can be found in Sorgente et al.2003, as in the paper references.

Section 3.2: P15,line16-18: I did not well understand what you mean by ‘the grid point whose depth.....has been excluded’. It corresponds to offshore areas where the total depth is very huge and then the first sigma layer thickness is larger than 1m. So

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these areas are not really in the Central Med.? Re: SCRM uses a bottom-following, sigma-coordinate system based on the transformation  $x^*=x$ ,  $y^*=y$  and  $\sigma=(zeta-\eta)/(H+\eta)$  where  $H(x,y)$  is the bottom topography and  $\eta(x,y,t)$  is the surface elevation (the transformed equations can be found in User guide of Princeton Ocean Model). This introduces a new set of independent variables that transform both the surface and the bottom into coordinate surfaces. In the present study the model has 30 vertical sigma levels. In the deepest areas of the Mediterranean, like in the Ionian Sea or south Tyrrhenian Sea (for example  $H=4000$ ), the depth of the first sigma level of the velocity field is at about 3 m, the second at about 4 m, and the third at 8.5 m. In this case we would have only two sigma levels which can contribute to the computation of MKE (eq. 7), EKE (eq. 8), etc. through eq. 12. Here we preferred to exclude these grid points because not sampled by a sufficient (in our opinion) number of sigma levels to well represent the surface layer, concerning the computation of the kinetic energies. Therefore deepest areas, not matching these requirements, have been excluded from the analysis.

Another point that could perhaps be a little more explained concerns the wind stress work. What is its significance? I understand that it allows looking at the relation between the wind and the current. Can you shortly explain some cases or give an example of  $W_s$  when the current is in the same direction of the wind or against the wind or orthogonal to the wind? Is there some relation with the surface Ekman transport? It can help the understanding of the description made in section 4.2.2 (page 22 line 4) in the Sicily sub-region for example.

Re: Thank you for this precious comment. It allowed to go further in depth to this argument, enriching the analysis and the discussion, and also to realize the presence of a small error in the routines computing the wind stress work. Old plots, differing a little from the new ones due to generally little larger values, have been replaced with new re-computed time-series of  $wsw$ . To the point: the wind stress work ( $W_s = \langle u \cdot \tau \rangle$ , where  $\langle u \cdot \tau \rangle$  ( $u, v$ ) is the surface current vector and  $\tau$  the wind stress vector)

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can be defined as the work done by the wind over the ocean. Using the wind stress work it is possible to calculate the wind energy input to the near-inertial motion [Alford, 2003] or the geostrophic circulation [Wunsch, 1998]. Positive values indicate a positive contribution to the flow of surface currents (positive work), while for negative values the wind stress acts negatively on the surface currents (negative work). Then this variable allows to easily evaluate (from its sign and magnitude) when surface currents are wind driven or not, and also to quantify this contribution. This is an important information that cannot be easily retrieved just by considering the wind stress time series.

Alford M. H. (2003), Improved global maps and 54-year history of wind-work on ocean inertial motions, *Geophys. Res. Lett.*, 30(8), 1424, doi: 10.1029/2002GL016614. Wunsch, C. (1998), The work done by the wind on the ocean general circulation, *J. Phys. Oceanogr.*, 28, 2332-2340.

We added the following statement, and related bibliography: ... (Pedlosky, 1996...; Zhai et al, 2007), representing the work done by the wind on the sea surface. Zhai, X., and Greatbatch, R. J., Wind work in a model of northwest Atlantic Ocean, *Geophysical research letters*, Vol. 34, doi: 10.1029/2006GL028907, 2007.

Section 3.3: You use AVISO  $\frac{1}{4}$  resolution SLA. It is relatively a coarse resolution for the Central Med. Maybe you could look at a better product of  $1/8^\circ$  resolution at (<http://www.aviso.oceanobs.com/>)

Re: Actually, the dataset used is the  $1/8^\circ$  one. This was just a typo, corrected.

Section 4: Poulain and Zambianchi (2007) used only 60% of drifters drogued at the surface. Maybe you have to explain a little their product because I am not convinced that it is comparable with the average layer of 5m thick.

Re: you are right, in fact the two estimations are not fully comparable for several reasons (among which: the percentage of surface drifters; the only partial representation of different dynamics furnished by drifters sampling, while the modeling tool homoge-

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neously samples areas with strong or weak dynamics; the climatological character of the Poulain work). Therefore we pointed out much more on a qualitative than quantitative comparison. Since our model is a sigma-layer coordinate model, we have chosen the first 5-m layer (derived from interpolation of the sigma layers to prescribed z levels) as representative of the surface layer. Taking into account that the comparison with drifters it is not a crucial point for the paper, we reworded the sentence as follows “We integrated our results over the upper 5m depth, that we consider representative for the surface layer. Our results have been qualitatively compared with EKE estimates derived from the drifters-based work of Poulain and Zambianchi (2007), which is the only observational reference available for the area.”

Section 4.1: P17, line25: What is the diameter of the Sidra Gyre, is it a cyclone or an anticyclone?, can you show it on Figure 2 as well as the ABV, MCC, etc

Re: the Sidra Gyre is a sub-basin permanent anticyclonic feature (see P17 line 21). We added its graphical representation to Fig.2 as requested. Its diameter is about 150-200 km.

Section 4.2: P19: can you try to explain your choice for the sub-domains of EKE-MKE-etc calculations. Can it be related to the general patterns of the circulations in this area? I propose to merge the figures to allow an analysis of the connexion between the different areas. It is not easy to look separately to the figures. I propose that Figure 5 shows 5 sub-figures (a) TKE, (b) EKE, (c) MKE, (d)  $\log(EKE/MKE)$ , (e)  $W_s$ , and inside each sub-figure should be drawn the corresponding time-series for the 4 areas. This approach is already partly done for the last part of the paper but should appear here too. Then when you start the description of the Sardinia-Tyrrhenian sub-region in Section 4.2.1 page 20, it will be easy to see/believe that ‘P20, line1: A appears as the most energetic area of the four’ before it is not possible to see it in present Figure 5. It could also allow some synthesis of the descriptions of Section 4.2.

Re: The sub-domains have been chosen on the basis of the general (basin and sub-

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basin scales) circulation and topographic constraints. A previous unpublished study (grey literature) based on EOF decomposition of surface T, S and velocity fields also provided synthetic information to define such sub-domains. As suggested, we plotted a new Fig.5 with 5 subplots. This actually furnishes a more synthetic information to the reader and facilitates the comparison of sub-domains. Wind stress work time series have been replaced with new re-computed (and corrected) estimations.

Section 4.2.1: P21, line 20-21: You write 'It seems to be part of a large anticyclonic gyre located on the southern side of the Tyrrhenian sea'. Can you confirm if this gyre is really present in the coarser large-scale model (show a map for example) to be sure that it is not a problem coming from the boundary conditions? Is it a permanent feature (during the 2-year period)? What is its diameter? Is it a different structure than the seasonal circulations described by Artale et al. (1994)? Finally, choose between a gyre or a current to describe this WNSC.

Re: We consider WNSC as a poorly known current flowing westward along the northern Sicilian coast. We guess that path and shape of this current would be part of a wider anticyclonic gyre. Thanks to your comment we realized that this feature was not so well explained in the manuscript, so we added a new figure to better discuss about this new features. The presence of the current and the gyre (with the limitations of the coarse model resolution) is evidenced also by looking the coarse model maps (attached at the end of this doc), so it does not seem an open boundary problem. However the study of this summer feature would deserve for sure a further insight. This gyre seems to have a high variability in its diameter, position and strength. The structure described by Artale is the SESG (South Eastern Sardinia Gyre).

Section 4.2.2: P22: See the previous comments to add PV in Figure 2 and a more detailed explanation of Ws.

Re: changed accordingly.

P23: I found very nice the comparison to satellite data highlighting the presence of PV.

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Re: we appreciate you appreciated.

Section 4.2.3: P23: I am surprise of the low value of the wind stress work because the bathymetry is not deep so should not constrain the circulation and thus I expected seeing a high impact of the wind on the surface circulation. Maybe I have not well understood the Ws significance again. Or maybe is the circulation on the plateau more linked to the winds in another area.

Re: As you pointed out, the wind stress work series suggest that the surface circulation over this region is not mainly related to the wind forcing, contrarily of what would be expected considering the shallow bathymetry. We would stress that this behaviour is true for the temporal and spatial scales considered in the analysis, i.e. monthly and sub-regional. It could be that at daily time scales, and in some area of this sub-region, the surface currents actually could have the expected behaviour, with a strong correlation with the wind forcing. We guess that this “anomalous” behaviour for such a shallow area could be due to the presence of some major currents, related to the flow of the AW, that are not strictly dependent by the wind forcing (AC, ATC, BATC, ALC) and that, at the same time, are the main responsible for the large EKE values we showed for this sub-domain (therefore also driving, together with the wind stress, the values of the wind stress work).

Section 4.2.4: The circulation in the southern part looks very complicated and it is interesting to look at your results in this area. But (P25) it is not clear if there are currents or just filaments around mesoscale eddies. A current has a vertical shape in velocity. Can you verify that you are really spoken about current?

Re: We are quite confident that such a complicated mesoscale circulation would also produce many intermittent filaments connected to eddies. In any case, considering the time scale of the analysis, such filaments (usually characterized by a short life-time) should not have a clear signature in the maps. In any case, as you suggested, by considering the vertical shapes, these structures show a vertical shape

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in velocity. A quick look for this can be given by looking the SCRM forecasts at <http://www.seaforecast.cnr.it/en/scrm.php>.

Conclusions Any comment on the wind stress work. The connexion between the ATC and the ALC is not evidenced by surface current analyses. Maybe the authors could look at depth if the ATC is still present and can become the ALC when reaching the Libyan coast. Sometimes stratification seasonal changes can affect the surface current signature.

Re: Concerning Wind stress Work and its relation with the dynamics over the Tunisian shelf, we changed the statement at p 1189, L5-9 as follows: “Although this is a shallow water area, the wind stress does not seem to be the primary source of energy at monthly scale. High EKE values seem mainly due to the baroclinic instabilities of the seasonal currents flowing eastward over the Tunisian shelf. The analysis of BEC indicates that this sub-region is characterized by a permanent energy conversion from EAPE to EKE. This can be explained taking in mind that in this area sudden current reversals take place. The effect of such reversals, in terms of enhancement of the variability, is increased by a flat and shallow bathymetry and by the unsettled advection of ATC over the wide shelf.” About the connection between ATC and ALC, we think that Figs. 3 and 13 show it quite well.

References Add Herbaut et al. (1998)

Re: added

Figures The latitude and longitude values are not visible and the character size has to be increased. The colorbars are too much close to the sub-figures. And the sticks on the isocontours are not visible.

Re: Changed accordingly

Figure 2 : Precise what are the full and dotted lines. The 'ALC' and 'SEGC' word/stick are not in the Figure but in the legend. see also the specific comments to draw the

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mesoscale features MCC, ISV, etc.

Re: please look at the comment above related to Figure 2

Figure 5: add 'panel C, the logarithm of EKE/MKE ( $\phi$ ) and its unit

Re: Done

Figure 6: remove 'The monthly ...5 m depth'. Add what is the month (November I guess). Area with 'EKE' (remove 'MKE and' EKE) larger than '100' (remove '80') ...

Re: We added November and changed 100. Furthermore we changed the indicated period from "January 2008 to December 2010" to "2008 to 2010" that is more corrected

Figures 9 and 12, 14: similar comments as for Figure 6

Re: Done

Figure 10, C: the colorbar seems to be in meter not in cm as written in the legend. The colorbar are not the same for panels C and D. Maybe the model results can be shift by 0.01m if it is mentioned in the legend to better see the features.

Re: You are right, there are several errors in the captions, among which the unit of panels C and D. Moreover, panel B is not elevation but velocities, while k490 units are obviously NOT cm. Colorbars of C and D have now been imposed equal (this does not changed the signature of the features, which remain the same as before)

Figure 12: EKE larger than '60' not '80'

Re: changed.

Figure 13: Lybian sub-region.

Re: changed.

Figure 17: where is the colorbar?

Re: added. Figure have been changed, enhancing BEC features and patchiness.

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Panel B has been changed to July, to show different behaviour in different seasons.

Word choice and typographical errors: Choose between eastern sub-basin or basin, again for the western Mediterranean P3, line 2: eastern and western Mediterranean sub-basins Compared to P6, line 10, line 20: Mediterranean basins

Re: chosen for sub-basin for eastern and western Mediterranean. Changed where necessary through the whole manuscript.

I think the unit is kg and not Kg for kilogramme (see p 11 for example)

Re: right is kg. Changed everywhere.

Section 2.1: P6, line 6 Béranger

Re: Done

P6, line 11: (AW) remove hereafter, it is a standard

Re: removed

P6, line 12: 2-layer without s

Re: changed

P6, line15: outflow not Outflow

Re: changed

P6,line21: intense current, not intensity

Re: changed

Section 2.2: P7,line6: few-kilometer long

Re: changed in “long few kilometers”

P7,line15: 2001; Onken et al., 2003; Béranger et al., 2004; Herbaut et al., 1998).

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Re: added Herbaut et al., 1998 but first in the list.

P8, line 17: Sorgente et al., 2003; Béranger et al. 2004)

Re: done

P8, line 22: lower frequency signals

Re: changed

Section3.1: P10, end: 5-day forecasts by Gabersèk

Re: changed accent

\*\*\*\*\* REFEREE # 2 General comments:

After careful reading of the suggestions given by the Referee #2 we have slightly changed the structure of the paper, without changing its main framework, in order to improve its reading. All the changes are described and justified in the “Specific comments” section. Number and structure of plots have been changed/improved (also following the suggestions of the Referee #1) in order to better point out the new features. Concerning the sub-regions we think that this kind of analysis allows to better focus on site-specific features, notwithstanding the risk of some redundancy. In any case we significantly reduced and synthesized the description and figures about sub-domains, also following suggestions of Referee #1, improving the readability of the paper.

Concerning this statement: “The comparison between the kinetic energy components in several parts needs further elaboration”, if we correctly interpret the thoughts of the Reviewer #2, we think that a further addition of comments and interpretations of the kinetic energy decomposition would make the readability dull. So we would avoid, if not strictly necessary, to add further elaborations. Finally we have improved the English in the text.

Specific comments:

Section 3.1 (Model implementation): You should comment on how the resolution of the atmospheric forcing (is it enough compared to the SCRM resolution?) and the boundary conditions are influencing the findings.

Re: This issue pointed out by the reviewer for sure deserves attention and is a central point in general for any model-based study. Nevertheless, this is a quite complicate issue to be solved in few words, statements or even in a section of a paper. It requires sensitivity experiments to be accomplished, dedicated data analysis, etc. that are slightly off topic for this paper and are more in the framework of a cal-val paper/activity. Certainly all of these could be arguments for separate papers (atm forcing/boundary conditions impacts). You can find some sensitivity study to surface forcing for our model in this discussion paper (<http://www.ocean-sci-discuss.net/3/637/2006/osd-3-637-2006.html>) which, and this was a pity, was not followed by a final paper. Concerning boundary conditions sensitivity, you can find further informations in Sorgente et al 2003, Oddo et al. 2008.

Section 3.2: Please explain, in BEC definition, why and how  $w'$  is derived from equation (1) (and not the model results).

Re: This is just a typo. The right equation is number 4 and not 1. Corrected in the text.

Why did you exclude points where the first sigma layer exceeds 1 m?

Re: SCRM uses a bottom-following, sigma-coordinate system based on the transformation  $x^*=x$ ,  $y^*=y$  and  $\sigma=(zeta-\eta)/(H+\eta)$  where  $H(x,y)$  is the bottom topography and  $\eta(x,y,t)$  is the surface elevation (the transformed equations can be found in User guide of Princeton Ocean Model). This introduce a new set of independent variables that transform both the surface and the bottom into coordinate surfaces. In the present study the model has 30 vertical sigma levels. In the deepest areas of the Mediterranean, like in the Ionian Sea or south Tyrrhenian Sea (for example  $H=4000$ ), the

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depth of first sigma level of velocity field is about 3 m, the second is located at about 4 m, while the third at 8.5 m. So, in this case we would have only two sigma levels which can contribute to the computation of MKE (eq. 7), EKE (eq. 8), etc. through eq. 12. In these cases, we preferred to exclude these grid points because not sampled by a sufficient (in our opinion) number of sigma levels to well represent the surface layer, concerning the computation of the kinetic energies. Therefore deepest areas, not matching these requirements, have been excluded from the analysis.

Section 4.2.2: Does the model reproduce the exchange flow at the Sicily Strait?

Comment: The inflow of MAW through the surface of Sicily Strait is well reproduced by the model, also the westward sub-superficial LIW flow. Please refer to Sorgente et al., 2003 (see references).

When comparing the model results with satellite observation, why are you using only one day and not the whole period or the month you are focusing on?

Re: We were interested in identifying features, like the PV, that have oceancolor (L2 products derived from visible bands of the EM spectrum: chlorophyll, k490 etc) or sea level anomaly signatures strongly variable over a month or longer. Moreover, they can strongly vary within a single week, or even (for oceancolor products) daily. Monthly averages smooth too much the isolines of any variables to put in evidence such well-defined mesoscale features. In these cases snapshots or at most daily means are preferred. Further we would like to remind the aim of such comparisons: 1) to demonstrate the ability of the model to reproduce small or intermittent mesoscale features like the PV; 2) to show the actual existence of such new features. A general validation of the model with satellite (SST) and in situ (CTD) observations has been accomplished in Olita et al. 2007, as in the references.

In several figures and parts of the text the authors compare MKE and EKE and then again describe the phi parameter. I think the second (or the first) is redundant.

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Re: Yes it could be. Nevertheless the presence of both can be useful because the time series are able to show the absolute magnitude of the two energies (that can be important to compare them with those of other sub-domains) while phi immediately allows to understand what is the driver (EKE or MKE) for a given area. Considering these aspects and that, mostly, we are talking about time series, we don't think that their removal is important for actually lightening the paper, while they can help to deeply and better understand the results (even if with a slight redundancy as you correctly noticed).

There are several typos and phrases that need rewriting: Title: "energy" instead of "energies"

Re: changed

Page 1162 – l.25: delete "the" from "the kinetic energy"

Comment: deleted

Page 1162 – l.26: "steady" instead of "stable"

Comment: we have changed with permanent, meaning permanent structures, always identifiable in the region where they can also fluctuate and not fix as steady could be interpreted.

Page 1163 – l.19: it's not "aerial" (!) it is "spatial"

Re: changed

Page 1163 – l.25: In the phrase "...scale of the Mediterranean..." delete "of the"

Re: deleted

Page 1164 – l.14: change "lacking" with "while there is lack of observations"

Re: changed

Page 1164 – l.19: I guess you not mean "poorly" but "were uncommon"

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Re: changed with were uncommonly

Page 1164 – l.28: delete “a” at the beginning of the line

Re: deleted

Page 1165 – l.6: replace “whom” with “which”

Re: replaced

Page 1166 – l.13: “tow-layer”

Re: changed layers in layer

Page 1166 – l.22: What do you mean by “intensity currents”?

Re: changed in intense currents as also requested by Referee 1

Page 1166 – l.25: “activity”

Re: changed

Page 1167 – l.7: Replace “origins” with “generates”

Re: changed

Page 1168 – l.28: Replace “from” with “of”

Re: changed

Page 1170 – l.14: Replace “denser” with “with more coverage”

Re: changed

Page 1171 – l.2: It’s not “negligee” (!), try “diminish”

Re: changed

Page 1175 – l.1: Replace “motion less” with “motionless”

Re: changed

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Page 1175 – l.4: “forms”

Re: changed

Page 1177 – l.10: Replace “approximate” with “relatively smooth”

Re: replaced

Page 1178 – l.18: Replace “could also be due to the other different factors” with “could also be due to other factors”

Re: replaced

Page 1179 – l.12: Replace “drawing” with “following”

Re: replaced

Page 1179 – l.13: Replace “path” with “cycle”

Re: replaced

Page 1179 – l.14-15: The phrase needs rewriting

Re: rewritten as The EKE is always larger than MKE (Fig. 4b) and dominates such a TKE seasonality following a similar cycle. Finally the MKE shows a smaller seasonal variability with an opposition phase period (EKE decreases while MKE increases) between May and July (relative maximum).

Page 1179 – l.20: Replace “probable” with “is probably related to”

Re: replaced

Page 1180 – l.11: Replace “overpass” with “exceeds”

Re: replaced

Page 1181 – l.7-8: The phrase needs rewriting

Re: rewritten as Then, generally, EKE maxima are always associated with high ener-

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getic flows like the AC and BTC. BTC strength appears mainly related to the eastward flow at the Sardinia-Tunisia section (Fig. 7) and, secondly, to the atmospheric forcing (Fig. 5b).

Page 1183 – l.19: “till” (?!)

Re: changed with up to

Page 1184 – l.16: “Highest kinetics” (?!), what do you mean by that?

Re: rewritten as Their kinetic energies are found high through the whole year and particularly in June

Page 1186 – l.19: Correct “...due to the fact that the core...”

Re: corrected

Page 1187 – l.12: Replaced “dislocated” with “located”

Re: replaced

Page 1189 – l.15: Replace “eddies” with “eddy”

Re: replaced

Page 1190 – l.7: What do you mean by “interested”?

Re: Changed to “take place”

There are several typos in the reference section.

Re: verified and corrected where necessary

Caption of Fig. 3 is wrong (A is not the kinetic energy).

Re: right, it is the mean flow. Corrected.

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