Answer to Reviewer # 1

V.M. Canuto and M.S. Dubovikov

First paragraph of the Report

• The Referee states that we "***do not attempt to validate the results based on high resolution models or observations",*** an assertion that does not correspond to the facts.

In fact, in Part I of our work (*Ocean Sciences*, 6, 1-15, 2010; doi:10.5194/os-6-1-2010), our sub-mesoscale model was amply validated against the available eddy resolving simulations. The validation included qualitative and quantitative tests. All high resolution simulations (Thomas, 2005; Thomas and Lee, 2005; Thomas et al., 2008; Mahadevan et al., 2008, hereafter MTF; see references in the paper) show that wind is a crucial factor in sub-mesoscales generation and that the most vigorous eddies are generated in the case of a down-front wind while in the case of an up-front wind, sub-mesoscale eddy generation is much weaker.

This simulation prediction is in full accord with the predictions of our model which in addition predicts other features. In particular, the model predicts that in the case of a down-front wind, whose direction is the most favorable for sub-mesoscale eddy generation, re-stratification by sub-mesoscales largely compensates the de-stratifying effect of the mean flow. This prediction was confirmed by the simulations of MTF.

It is worth noticing that our prediction was published before Dr. Mahadevan sent us a preprint of the MTF paper.

Furthermore, we validated our model quantitatively. In fact, we tested it against the available simulation data on the sub-mesoscale vertical buoyancy flux which are of two types: with and without wind (Capet et al., 2008, C8; Fox-Kemper et al., 2008). Both model tests are discussed in detail in Part I where we showed a satisfactory quantitative agreement between simulation data and model predictions.

• The Referee states that we ***" do not present any results from using the parameterization in OGCMs.***
He is absolutely correct. We have neither the expertise nor the capability to do so. Our task in Part I was to *derive and assess* a sub-mesoscale parameterization so as to give the reader the complete picture of which assumptions went into the derivation of the model results and how the model fared vis a' vis eddy resolving simulation data available to us. We have done both parts and our job terminates at that point. OGCM experts can take over from here.

Second paragraph of the Report

• This paragraph of the report contains incorrect assertions. The Referee states that it is necessary to account for the variations of N and h over mesoscales. As it is well known, such variations are truly small in comparison with their large scale counterparts. A glance at Fig.11 of C8, which presents their results of mesoscale (and even sub-mesoscale)resolving simulations, is quite useful. The black curve (corresponding to the lower boundary of the mixed layer) shows no sizeable structural variation of the depth of the mixed layer over the scale of mesoscales (say 100km). The reason is that N and h are determined by the atmospheric scales of wind, heat and evaporation-precipitation fluxes which are considerably larger than mesoscales.

• Next, concerning relation Eq.A2 and the validity of the axisymmetric approximation for mesoscale eddies, we refer to observations: "*mesoscale eddies are water-mass anomalies with* ***nearly circular*** *flow around their centers which move through the background water at speeds and directions inconsistent with background flow*" (Richardson, 1993, Tracking ocean eddies, *American Scientist*, **81**, 261). This observations based fact justifies the axisymmetric approximation that we used.

• The last statement in this paragraph "*the parameterization equations do not appear to be closed since the tracer slope* ***s*** *in Eqs.(9e,f) depends on the mesoscale buoyancy gradient*", is not correct. **First**, the slope **s**, as it follows from its definition in Eq.(9f), is the slope of isopycnals averaged over large scales, not "**a tracer slope which depends on the mesoscale buoyancy gradient",** as stated in the Referee's report. Thus, the parameterization given in Eqs. (9), is perfectly closed since all the terms are expressed in terms of the large scale, resolved fields.

Third paragraph of the Report

Several comments are in order here.

• Concerning the Referee's assertion about the notation we employed, we must point out that previous authors, for example Capet at al. 2008, have used similar notation and in footnote #1 we have given the translation between the two notations. If the referee and the readers of JPO, where C8 appeared, had no troubles with that notation, they will have no troubles with our notation which is very similar. Even if we used exactly the same notation, the level of apparent, formal complexity would not change.

The truth of the matter is that we have to deal with mean variables, mesoscale variables and sub-mesoscale variables and thus, by necessity, three different groups of symbols must be used. One must consider mesoscale and sub-mesoscale fields which we denote with a prime and a double prime. In addition, one needs two kinds of averaging, one over large scales and one over small scales. Therefore, the correlation functions implying mesoscales fields and those implying sub-mesoscales (e.g., mesoscale and sub-mesoscale fluxes), require different notation. Even a simple list of such dynamical variables shows that a rather complex notation is unavoidable. That is the nature of the problem.

However, in the summary of the results, we have purposely simplified the notation in a way that the results are as simple as one can possibly make them. Anyone looking at Eqs.9a-f will agree that the notation is indeed quite transparent. This simplification is possible because in the final results we do not need the intermediate variables which were indispensable in the derivation.

• the Referee considers "curious" that we designate a tracer with "tau"; we are not the first ones to do so, see for example Gent et al. (JPO, 1995, **25**, 463).

• as for the wind stress, it is a **vector** and is designed by a highlighted "**tau**" and so no real confusion is actually possible.

In summary, we have proven that all the criticisms of the Referee have no scientific basis.