Answer to Reviewer # 2

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The report focuses on one single item, to disprove our relation A5 which reads:

$$\overline{\overline{\nabla_H b'' \cdot \nabla_H \tau''}} \approx \nabla_H \overline{\overline{b}} \cdot \nabla_H \overline{\overline{\tau}} \tag{1}$$

where a double prime denotes mesoscale fields and double bar denotes averaging over a coarse resolution grid size $\Delta x \sim 100\text{km}$. Furthermore, τ, b represent an arbitrary tracer and the buoyancy field, respectively. For the case $\tau = b$, using the spectrum $b''^2(k) \sim k^{-2}$ shown in Fig.(6) of Capet et al. (2008,C8), paper I (not paper III, as R2 misquotes), R2 derives his relations (11), (12) which yield:

$$\overline{\overline{|\nabla_H b''|^2}} \,/\, \left|\nabla_H \overline{\overline{b}}\right|^2 \approx \Delta x / L_f \tag{2}$$

where $L_f \sim 1\text{km}$ is the front width of sub-mesoscales. Thus, R2 concludes that ratio (2) is of the order of 100 which is inconsistent with our relation (1). That is the gist of R2 report.

Our answer is in two parts: **first,** we show that R2's relation (2) is inconsistent with the results in Fig.10 of C8 while our result (1) is consistent with C8; **second,** we discuss the wrong points in the R2's derivation of his relation (2) above .

First, in their Fig.10, C8 present (among others) the profiles of the large scale and mesoscale velocity fields. The former has a typical structure consisting of an Ekman layer (within the depth ~20 m), a transition zone (with the thickness ~20m), and a geostrophic component below ~40 m depth. From the slope of the mean velocity profile in the geostrophic regime in Fig.10 and using $f \approx 0.8\, 10^{-4} s^{-1}$, the horizontal mean buoyancy gradient is estimated to be:

$$\left|\nabla_H \overline{\overline{b}}\right| \approx 4 \times 10^{-8} s^{-2} \tag{3}$$

One may expect that approximately the same $\left|\nabla_H \overline{\overline{b}}\right|$ occurs in the mixed layer (ML) which is 40m deep. Indeed, from the lowest two panels of Fig.11 of C8, in the ML, we deduce a values of $\left|\nabla_H \overline{\overline{b}}\right|$ which is approximately the same as in (3) when averaged over the basin. As for the mesoscale $\nabla_H b''$, we deduce it from the profile of the mesoscale velocity shown in Fig.10 of C8 since the mesoscale velocity is geostrophic. As one can see from the Fig.10, in the upper

30 m of the ML, the slope of the profile of the mesoscale velocity is considerably smaller than that of the mean velocity which implies that:

$$|\nabla_H b''| < |\nabla_H \overline{\overline{b}}| \tag{4}$$

However, within the last 10 m of the ML depth (which is about 40 m deep), from C8 Fig.10 we deduce a relation opposite to (4). Once we average over the ML, we obtain:

$$|\nabla_H b''| \approx |\nabla_H \overline{\overline{b}}| \tag{5}$$

which is consistent with our relation (1) and in obvious disagreement with R2 relation (2).

Second, we point out what went wrong in R2 analysis that lead to (2). The later results was derived from R2 assumption that the spectrum $b^2(k) \sim k^{-2}$ is valid for both large scales and mesoscales. However, as C8 Fig. 6 (left panel, middle row) shows, at

$$k < 2 \times 10^{-5} m^{-1} \qquad\qquad L>50km \tag{6}$$

which corresponds to scales larger than mesoscales, the spectrum $b^2(k)$ becomes much steeper thus invalidating relations (4) and (9) of R2 and, as a consequence, R2 relation (2).

The above arguments disprove R2 main criticism.

Further comments.

We disagree with R2's statement that our relation (A5) (i.e. Eq.(1) above) is "*the crucial aspect of the averaging*". Indeed, possible corrections in the numerical coefficient in (1) cannot alter our main result that the crucial factor in the sub-mesoscale parameterization, Eqs. (9a-f), is the wind stress, a conclusion in full agreement with available numerical simulation results. Still, R2 correctly points out that we derive relation (A5) within the mixing length approximation (MLA) which considers all mesoscale spectra to be concentrated around the deformation radius scale. It is indeed true that this approximation is the simplest one that allows us to account for non-linear terms in the primitive equations. Before our work we are not aware of works in mesoscale modeling that accounted for the non-linear terms while the linear approximation has a vast literature more than half of century old, beginning with the celebrated work by Eady to the more recent linear analysis by Killworth (2005).

In fields other than oceanography, the MLA has been widely applied beginning with its inception in 1928 when Prandtl first suggested it for flow with a Prandtl number larger than unity as the ocean certainly is. It has been applied for more than forty years with less of an a priori justification but with more than satisfactory results, to study convection in stellar interiors. Admittedly, the MLA is far being fully satisfactory. Nevertheless, within the MLA we were able to derive a set of quite satisfactory results on mesoscales and sub-mesoscales which cannot be obtained within the linear approximation. In particular, relation (1) which compares favorably against the C8 simulation results, as we just have discussed. A further list of some other results on mesoscales is presented in our recent paper (Canuto et al., 2010).

Finally, we need to comment on R2 remark that our "paper is difficult to read".

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 presented in relations (9a-f), 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 and easy to implement in coarse resolution OGCMs.

This simplification is possible because in the final results we do not need the intermediate variables which were indispensable in the derivation.

In conclusion, we have shown that the key criticism of R2 is based on an assumption about the spectra that is not consistent with the simulation data.