Reply to the FK second Comments

In our Reply to FK's first round of Comments, we touched upon several important issues summarized in **Overview of the problem**. Among those issues were**:**

a) the uniqueness of our work in the sense that no other model exists today in the literature that provides an analytical expression easy to use for the vertical flux for an arbitrary tracer under arbitrary buoyancy and wind,

b) the model predictions were confirmed by the MFT work which appeared after our work was submitted,

c) the only other model in existence is the FFH which does not have wind**,**

d) FFH is not for a general tracer but for buoyancy only,

e) the FFH model cannot be used in OGCMs but,

f) FFH can be used to test the predictions of our model in the limits c)-d) just mentioned.

In his new Comments, which we welcome, FK deals only with issue **f)** and raises the following three items.

I. Limits of applicability of our submesoscale (SM) parameterization.
II. Length scale in our SM parameterization.
III. Applicability of the FFH model to flows with the wind stress which is absent in the FFH parameterization.

Item I. FK presents five points a-e

a) FK insists that the upper limit Ri~25 for the applicability of our parameterization is not present in the original version of our paper.

It is a safe rule to assume that when a referee misinterprets some issues of a paper, chances are that the authors were nor sufficiently clear in their presentation. This is indeed the case and we shall clarify the issues not only in this reply but in the manuscript as well.
Formally, FK is correct, the limit Ri~25 is not explicitly stated in the original version. However, it is equally true that it follows straightforwardly from the restriction Ro ≥ 1 appropriate for SM that we discuss below Eq.(4d) and which we impose in order to simplify the analytical derivations. From $\mathrm{Ro} \geq 1$, one can immediately obtain Ri<25 using the following relations:

$$\mathrm{Ro}^2=\frac{u'^2}{f^2\ell^2}, \quad \ell=r_s=\frac{Nh}{\pi f}, \quad \frac{u'^2}{\bar{u}^2}=x(\mathrm{Ri}), \quad \mathrm{Ri}=\frac{h^2N^2}{\bar{u}^2} \tag{1}$$

from which we derive that:

$$\mathrm{Ro}^2=\pi^2 x(\mathrm{Ri})\mathrm{Ri}^{-1} \tag{2}$$

Using (**14c**), one finds the limit Ri~30 which corresponds the limit Ro~1. *Thus, from the condition* $Ro \geq 1$ *we obtain* $Ri \leq 30$.

In the paper we did not discuss the upper limit of Ri for the two reasons: first, values of Ri>30 are of no interest to a mixed layer study and second, we believe that our result (14f) has the correct asymptotic behavior at large Ri, even at Ri>30. At the same time, we believe that in this region the FFH data are contaminated by noise due to gravity inertial waves with periods less than one day which must be filtered out before computing SM fluxes. The waves are generated numerically due to the instability of the numerical procedure which is only partially overcome by the vertical turbulent diffusivity/viscosity. We observed analogous effects many times.

b) Below Eq.(4d), we really discuss the condition Ro~1: "*However, since in the submesoscale regime typically Ro~1, one must consider the complete expressions (4c)*".

Next, FK writes: "*Indeed it is suggested that (4c) will naturally become (4d) as Ro becomes small*" .This is a misinterpretation. In reality, in the paper we wrote that (4d) follows from (4c) for ***any Ro*** and that for small Ro, both (4c) and (4d) yield the geostrophic result $-ik\rho^{-1}p'$.

c) Certainly, in all results shown in Fig. 2 of FK's previous Comments, the ratio K_E/K(mean baroclinic)> 1. The limit K_E/K(mean baroclinic)=1 attains at the lower boundary of the Ri region in which the FFH data are presented. This limit in our model yields Ri=1.5 while in the FFH simulation is close to Ri~1. The latter is clear from Figs. 3,5 of the FFH paper in which the authors present the evolution in time of Ri and of eddy kinetic energy K_E. From these figures, one can obtain the dependence K_E(Ri) which shows that for Ri>O(1), the function K_E(Ri) grows with Ri and that K_E/K(baroclinic)~1 attains at Ri~1 which is in full agreement with our result. As for larger Ri, both our model and FFH simulations show an increase of the above ratio in agreement one with other. It follows that FK's assertion that we "*claim that larger Ri range does not satisfy this condition (* K_E */K(baroclinic)> 1)*" is a misinterpretation rooted in the FK's misreading (repeated in other places of FK's latest comments) that the restriction Ri<25 is derived from the condition K_E/K(mean baroclinic)>1. *In reality, Ri<25 is obtained from the condition* $Ro \geq 1$ *while from the condition* K_E */K(mean baroclinic)>1, we obtain Ri>1.5.*

d) this issue was discussed at the end of issue a)

e) this issue was discussed in the first part of issue a).

Item II

In the often used heuristic approaches, there are several definitions of the length scale ℓ which differ by factors of order unity. However, this not case in our work since all the results are derived after transforming the SM dynamical equations into an eigenvalue problem which contains the non-linear terms and which is then solved. In the limit:

$$K_E > K(\text{mean baroclinic}) \qquad (3)$$

we have obtained the following result for the turbulent diffusivity:

$$\chi \approx r_s K_E^{1/2} \qquad (4)$$

which is related to the turbulent viscosity by the turbulent Prandtl number. We interpreted (4) as $\ell\, K_E^{1/2}$ with the length scale $\ell = r_s$ by definition. Thus, in our SM parameterization we have no ambiguity in the choice of the length scale.

Item III

Mahadevan et al. (2010, MTF) stressed three issues:

a) strong dependence of the SM vertical flux on the wind stress,
b) strong cancellation of the re-stratification by SM and de-stratification by the mean flow in the case of a strong down-front wind ONLY.
c) from b), MTF suggested that the residual flux is in a qualitative agreement with the FFH parameterization.
FK quotes and highlights only c). *This is however far from being a proof and even less a statement that FFH can be used in the presence of wind*. In fact, in the general case, the SM flux depends strongly on the strength and direction of the wind, which cannot be accounted for by FFH since it contains no wind to begin with. The critical cancellation process in b) is not contained in the FFH model by definition.

Finally, on the more general questions dealt at the beginning of FK's second Comments, we believe that our model satisfies all the requirements of a genuine scientific theory since it is derived from solving the SM dynamical equations in the limit Ro=O(1), and adopting a model of the non-linearities tested in a series of eight papers that appeared in *The Physics of Fluids* in the 96-98 period. The tests presented in those papers contained no adjustable parameters and yet, embraced a large variety of different turbulent flows.

The SM model that we have constructed and presented makes specific quantitative predictions about the very complex physics of SM under arbitrary wind and buoyancy, predictions that may be verified or falsified by future data.

When we wrote our manuscript, the MTF paper did not exist. Several of the predictions made by our model were later confirmed by the MTF data, as we discussed in our previous reply to FK.