Reply to B. Fox-Kemper

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Overview of the problem

OSD provides the unique opportunity that the exchange between the authors and the referees is now open to the whole community. We thus believe that the latter will greatly benefit if the authors, before answering the issues raised by the referee, highlight the key features of the problem under study, which in this case is the:

- *parameterization of the Sub-Mesoscale (SM) flux for an arbitrary tracer under a wind of arbitrary strength and direction for use in OGCMs*.
- Mahadevan, Tandon and Ferrari, JGR, 2010 (MTF), have written that "Parameterization of the circulation induced by SM eddies in the presence of wind forcing is required in climate models in order to simulate the re-stratification correctly"**.** The implication is that such a problem has not yet been solved.
- coarse resolution OGCMs cannot numerically resolve SM. Therefore, one needs an analytic representation of the SM fluxes *of an arbitrary tracer (T,S and a generic concentration field) under arbitrary wind conditions (meaning direction and strength). The parameterization must account for non-linear interactions.*
- the parameterization must: **first,** be valid for an arbitrary tracer. A model for buoyancy is not sufficient since the latter cannot describe an important ingredient such as CO2; **second,** include a wind of arbitrary direction and strength since recent studies (e.g., MTF) have show that its effect on SM fluxes is large and because forcing in future climates is likely to be quite different from today's; **third,** account for the SM non-linear interactions.
- to derive the parameterization needed in OGCMs, there are two routes: a) numerical simulations and b) analytic treatment of the problem.
- in this work we present the result of option b), specifically, an *analytic expression for the SM flux for an arbitrary tracer (not only buoyancy) under arbitrary wind conditions. The model includes non-linear interactions and has no adjustable parameters.*
- a critical issue concerns the assessment of any model predictions vs. data. The following two conditions must be satisfied:

a) a model must be able to **reproduce** existing data,
b) a model must **predict new features** to be assessed when new data become available.
c) as we have shown in the paper, our model satisfies both conditions a)-b).

Reply to B. Fox-Kemper (cited as FK)

Since progress in science is incremental, we expected FK to welcome our work since it represents a non-trivial progress with respect to his own work (FFH, JPO, 2008).

The FFH model treated *buoyancy only and no wind and thus could not provide what OGCMs need*. We treated the more general case needed in OGCMs, that of an *arbitrary tracer and arbitrary wind.*

We were therefore surprised to see that most of FK's report was devoted to discussing the limited FFH model "*I will constrain this review to address issues with my and my colleagues own work*" .

There are two physical processes that must be included in any SM model to make it usable in OGCMs: **tracer rather than buoyancy and wind rather than no-wind**.

As for the **tracer,** FK never mentions it although it is a very important for OGCMs. It must be stressed that it is not possible to use the FFH model for buoyancy (an active tracer) to describe a passive tracer. On that issue alone, FK should have welcome our work as an important generalization of FFH.

As for the **wind,** FK writes that we "*correctly asserted that the FFH simulations are limited in that they do not have wind stress*", but then he claims that "*the FFH model does work in the presence of winds".*

As for the last statement, our comments are as follows:

First, in the paragraph below Eq.(15e), we compared the z-profile of the vertical buoyancy flux from the simulations by Capet et al. (2008) with wind with that obtained from the FFH formula and the results are as follows. If one substitutes Capet et al. results:

$$\nabla_H \overline{b} \approx 0.5\cdot 10^{-7} s^{-2} \text{ , mixed layer depth H=40m} \qquad (1)$$

into the FFH formula for the vertical flux, one obtains that the maximal vertical buoyancy flux is:

$$F_v(\mathrm{FFH}) = 2.4\cdot 10^{-9}\, m^2 s^{-3} \qquad (2)$$

to be compared with:

$$F_v(\text{Capet et al.}) = 2\cdot 10^{-8}\, m^2 s^{-3} \qquad (3)$$

Thus, contrary to FK's assertion, Capet's et al. results with wind exceed FFH fluxes by an order of magnitude.

Second, the z-profile of the SM vertical buoyancy flux obtained by Capet et al. (2008) in the presence of wind, is noticeably different from that predicted by the FFH model with no wind. The difference is especially evident in the $\partial_z F_V$ profile. In fact, FFH predicts a profile that is anti-symmetric with respect to the middle of the ML, while the profile in the presence of wind, as it follows from Fig.12 of Capet et al., is very far from having such a feature.

Third, as shown in the recent work by MTF, the effect of wind is quite complex and multifaceted as the following results show:

Strong downfront wind:

a) the stratification due to the mean flow and SM "*have opposite sign on average and largely cancel each other" (line 283 of MTF)*
b) "*the eddies are more vigorous than in the case without winds"* (line 234 of MTF).
c) at the end of the caption of Fig.3, MTF conclude that *"in the case of downfront winds, the mixed layer eddies become more intense as the wind stress is increased"*.
d) *i*n addition, in lines 318-319, MTF conclude that the SM streamfunction "*increases with the strength of the downfront wind"*.

Strong upfront winds:

a) MTF found that: "*When an upfront wind stress is applied to the model, restratification proceeds faster*" (line 286).
b) an analogous statement can be found in MTF Conclusions: "*Restratification is speeded up when the wind stress is upfront"*.
Conclusions:
- It is hard to see how FFH's parameterization that contains no wind could encompass all these features, as FK claims.
- by contrast, in **E)** we discuss how our model reproduces MTF results.

Other claims by FK that require comments are as follows.

A) *FK claims that in the no wind case, our model contradicts FFH data.*

Our comments are as follows.
First, to prove his point, FK used the unorthodox procedure of applying our model outside its region of validity.
Second, within the limit of applicability of our model,

$$1.5<\mathrm{Ri}<25 \tag{4}$$

our analytic formula (red curve in FK's Fig.2) clearly reproduces the FFH simulation data even better than the heuristic FFH fit (blue curve in FK's Fig.2).
Third, for completeness sake's, the derivation of (4) is as follows.

The general condition of validity of our model, expressed by Eq.(6c), is that *the SM kinetic energy exceed that of the baroclinic component of the mean flow*. In the particular case of no wind, this translates in the condition $\mathrm{Ri} \geq 1.5$ derived in Eq.(14d).

The second condition (discussed below Eq.4d) requires that the SM eddy Rossby number $Ro=u'/fr_s$ be of order unity or at least not much smaller than unity (r_s=NH/f is the Rossby deformation radius of the ML). This translates into the condition that $Ri^{1/2}$ must not be very large. However, what is "not very large" is difficult to determine on analytical grounds alone. One may expect that the lower limit of Ro be about 0.2-0.3 which correspond to Ri~10-25. Indeed, if we look at Fig.2 of FK's comments (updated results FFH simulations), one observes that in the interval $1.5 < Ri < 25$, *our model (red curve) without a single adjustable parameter, describes the simulation data even better than the FFH blue curve.*

It must be further stressed that values of Ri>25 are completely irrelevant to the ML as the history of ML parameterization from the work of Mellor-Yamada in the 80' to the most recent development, clearly shows.

B) *FK disputes the appropriateness of the SM deformation radius* r_s *in our model.*

The claim is that we have borrowed the length scale from the linear analysis and the second claim is that such a choice is incorrect because the real dynamic regime of SM is non-linear.

Our comments are as follows **.**

First, the presence of r_s in our model *is not an assumption at all.* It results from solving the eigen-value problem derived from the non-linear mesoscale equations (Canuto and Dubovikov, 2005, CD5). Even though the equations and boundary conditions of the linear and non-linear problems are different, the results for the appropriate length scale of non-linear problem coincide within a factor O(1) with that of the linear case. This conclusion is confirmed by numerical simulations (e.g., Thomas et al., 2008), including those by FFH. In fact, as discussed in Appendices A and B, in our model the length scale ℓ is determined by the energy spectrum. On the other hand, in the FFH simulations, as it is apparent from their Fig.9 or from the right picture of Fig.1 of the present FK report, the scale characterizing the energy spectrum is close to Stone's length scale which is of the order of the deformation radius.

What is most surprising is that FK uses his figures as an argument against the length scale r_s in non-linear dynamics while his figures are actually in favor of it!

Finally, as MTF noticed "Observation by Hosegood et al. (2006) show that upper ocean fronts are often baroclinically unstable and exhibit variability at scales of the order of the ML deformation radius ".

Second, as in the previous section, FK created a non-existing difficulty. In fact, ***even if*** the length scale in our model were unknown, it would be the only adjustable parameter in the whole model which could be fixed, say, from comparison with the FFH simulations. Since with this parameter, our model **reproduced** existing data (Capet et al., 2008) and **predicted** behaviors that were later proven correct by the work of MTF (see section **E**) below for details), we conclude that even if we had one putative unknown length scale parameter, our model represents a significant advance in parameterizing SM, a performance clearly superior to the mismatch by an order of magnitude if one applies the FFH model.

C) FK states that our *"result cannot be claimed to be new.."*

Our comments are as follows.
Our work contains one "*result*", the parameterization of SM. To prove the novelty of it, we refer to MFT who state that such parameterization is not yet available "*Parameterization of the circulation induced by SM eddies in the presence of wind forcing is required in climate models in order to simulate the re-stratification correctly*".
What we have presented is the first analytic expression for the SM flux for an arbitrary tracer (not only buoyancy) under arbitrary wind conditions.
If our "result" is not new, as FK claims, the readers and we would be grateful to know where and by whom was the SM tracer flux in the presence of arbitrary wind expressed with an analytic formula and where and by whom was such an expression assessed against available SM resolving data.

D) FK's asserts that *"the other aspects of the paper do not seem new and are reproductions of the earlier works of Canuto & Dubovikov on the mesoscale adapted to submesoscale (1997, 2005, 2006)"*.

Our comment is quite straightforward..
In the work cited by FK, we treated the non-linearities in the limit of small Rossby number $\mathrm{Ro} << 1$ as appropriate to mesoscales, while in the present paper we considered the regime $\mathrm{Ro} \sim 1$ as appropriate to submesoscales. There is a fundamental physical difference between the two regimes which seems to have escaped FK's attention..

E) FK recognizes that our comparisons with Capet et al. data "*are encouraging*" but at the same time he writes that*" a comparison to simulation data is a necessary, not sufficient, proof of particular parameterization."*

Our comments area s follows**.**
As the philosopher of science K.Popper taught us, no scientific theory of any kind can ever be proven but only falsified. Thus, FK's request of a "proof" contravenes well established and generally accepted canons of science. A scientific theory is such only if it makes *predictions* and thus can be "*falsified*", as Popper further suggested. A theory that makes no predictions, cannot be falsified, and is therefore not a scientific theory.
Our model's reproduces data that already existed (Capet et al., 2008) and predicted others that appeared after we submitted our manuscript.
To be precise, on Sept. 29, after we had posted our paper on the website, Dr. A. Mahadevan kindly sent us the manuscript *Rapid changes in mixed layer stratification driven by submesoscale instabilities and winds* (Mahadevan, Tandon and Ferrari, MTF).
Our model would have been falsified if its predictions did not satisfy MFT data.
Since the process of predictions vs. data comparison is a fundamental issue for any scientific model, and given the nature of FK's statement **E**) above, we have no choice but to stress the following important items:

- **strong downfront wind.** In this case, the wind driven Ekman flow destratifies the mixed layer while SM re-stratify it so that the restratification due to the mean flow and

SM "*have opposite sign on average and largely cancel each other" (line 283 of MTF).* This conclusion was stated in both Abstract and Conclusions of MTF which means that the authors considered it worth stressing.

Our Eq. (9c) predicted the same result.

To prove this conclusion, we present the following argument. Consider the two contributions to the mean buoyancy equation: $\partial_z F_V^b$ is the SM part and the second is $\tilde{\mathbf{u}} \cdot \nabla_H \overline{b}$ (where $\tilde{\mathbf{u}} = \overline{\mathbf{u}} - < \overline{\mathbf{u}} >$ is the baroclinic component of the mean velocity $\overline{\mathbf{u}}$ while $< \overline{\mathbf{u}} >$ is the averaged over the mixed layer depth, i.e., the barotropic component of $\overline{\mathbf{u}}$). Eq.(9c) expresses the approximate cancellation of these two components. To obtain the corresponding contributions to the re-stratification, one need to z-differentiate the expressions contained in the right and left hand sides of (9c). Since in the mixed layer $\nabla_H \overline{b}$ is almost z-independent, in the z-derivative of the left hand side of (9c) one may substitute $\tilde{\mathbf{u}} \to \overline{\mathbf{u}}$ and conclude that the contributions to the re-stratification by the mean flow and SM almost compensate each other, which coincides with MTF's result which, as stated above, was unknown to us at the time we posted our manuscript. We further stress that the agreement between MTF numerical results and our analytical ones, is not only a qualitative test of our model but a quantitative one as well.

- In the case of a **strong downfront wind,** MTF found that "*the eddies are more vigorous than in the case without winds"* (line 234 of MTF). At the end of the caption of Fig.(3) MTF conclude that "*in the case of downfront winds, the mixed layer eddies become more intense as the wind stress is increased"*. In addition, in lines 318-319, MTF conclude that the SM streamfunction "*increases with the strength of the downfront wind"*.

Our model (9a,c) and (10a,b) predicted the same feature.

- As for **upfront winds**, MTF found that: "*When an upfront wind stress is applied to the model, restratification proceeds faster*" (line 286). An analogous statement can be found in MTF Conclusions: "*Restratification is speeded up when the wind stress is upfront*".

Our sc.7 predicted the same results.

Indeed, in our model, SM may be generated only if its vertical buoyancy flux is positive and has a negative second derivative. This means that SM always restratify the mixed layer while upfront winds do the same. Thus, our result coincides with the MTF: in the case of an upfront wind, the mean flow and SM "*are both thermally direct and reinforce each other to hasten the rate of restratification"* (line 289).

- Our model further predicted that in the case of a very strong upfront wind, SM eddies are not generated. It would be quite useful if simulation modelers could check this prediction.
- We stress that the agreement between the MTF results and ours show a strong dependence of the SM fluxes on both strength and direction of the wind. Given the

multifaceted role played by the wind in the re-stratification induced by SM, and since wind is absent in FFH, FK's claim that "*the FFH model does work in the presence of winds*", is manifestly untenable.

F) Conclusions

1) OGCMs need an analytic expression for the SM fluxes for an *arbitrary tracer under arbitrary wind conditions* so as to account for both active (T,S) and passive fields (e.g., concentrations, CO_2, etc).
2) the only parameterization available today is the one by FFH which however corresponds to **buoyancy only and no-wind**, two limitations that fall short of what is required by OGCMs
3) in the no wind case, in the range of validity of our model discussed in **A**), our analytic formula reproduces FFH data even better than the fit suggested by FFH (see Fig.2 of FK),
4) to best of our knowledge, what we present is the first and only analytic expression for the vertical SM flux of an arbitrary tracer in the presence of wind of arbitrary strength and direction.
5) the model reproduced existing data and predicted features that were later confirmed by newer results by MFT that were unknown to us when the model was submitted to OS.
6) the model can easily be implemented in any OGCM.

References

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