Continued Discussion of the Theory by Canuto and Dubovikov

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I am glad that in the last reply, Canuto and Dubovikov raised the scientific philosophy and importance of falsifiability so clearly framed by Popper. Invoking his analysis raises the level of our discussion and applies here well as an Overview of the Problem. Thus, I add a lengthy quotation from Popper (1998).

1 Overview of the Problem

> These considerations led me in the winder of 1919-1920 to conclusions which I may now reformulate as follows:
>
> 1. It is easy to obtain confirmations, or verifications, for nearly every theory–if we look for confirmations.
> 2. Confirmations should count only if they are the result of *risky predictions*; that is to say, if, unenlightened by the theory in question, we should have expected an even which was incompatible with the theory–an even which would have refuted the theory.
> 3. Every 'good' scientific theory is a prohibition: it forbids certain things to happen. The more a theory forbids, the better it is.
> 4. A theory which is not refutable by any conceivable even is non-scientific. Irrefutability is not a virtue of a theory (as people often think) but a vice.
> 5. Every genuine test of a theory is an attempt to falsify it, or to refute it. Testability is falsifiability; but there are degrees of testability: some theories are more testable, more exposed to refutation, than others; they take, as it were, greater risks.
> 6. Confirming evidence should not count *except when it is the result of a genuine test of the theory*; and this means that it can be presented as a serious but unsuccessful attempt to falsify the theory. (I now speak in such cases of 'corroborating evidence'.)
> 7. Some genuinely testable theories, when found to be false, are still upheld by their admirers–for example by introducing *ad hoc* some auxiliary assumption, or by reinterpreting the theory *ad hoc* in such a way that it escapes refutation. Such a procedure is always possible, but it rescues the theory from refutation only at the price of destroying, or at least lowering, its scientific status. (I later described such as rescuing operation as a '*conventionalist twist*' or a '*conventionalist stratagem*'.)
>
> One can sum up all this by saying that *the criterion of the scientific status of a theory is its falsifiability, or refutability, or testability.*

2 Reply to the Reply by Canuto and Dubovikov

Both the Fox-Kemper et al. (2008) (FFH) and Canuto and Dubovikov theories offer testable prohibitions of certain behaviors (points 3 and 5). However, the theory of Canuto and Dubovikov is intended to be more broadly applicable (including wind, for example), while the FFH parameterization certainly leaves room for improvement by measure 3 above. It *does not forbid any behavior* in the following cases:

- Wind (the theory is founded upon empirical consistency only with simulation results without wind)
- Linear eddies, i.e., before nonlinear saturation has occurred (the theory is based upon a nonlinear saturation assumption)

Furthermore, the theory of Canuto and Dubovikiov is free of tunable parameters, while the theory of FFH has an empirically-determined factor C_e. It is certainly true that the FFH theory is thus deficient by omission, yet it is empirically accurate when it is validly applied.

In the preceding discussion, Canuto and Dubovikov show that they are likely in violation of Popper's principles 1 and 6. As evidence, note:

- The upper limit ($Ri < 25$) was not present in the first version of the paper. They only suggest that $Ri > 1.5$ is required.
- Their claim that relation 4d requires $Ro = \frac{u'}{fr_d} = O(1)$ seems suspect and certainly is not laid out in the first version of the paper. Indeed it is suggested that 4c will naturally become 4d as Ro becomes small.
- *All* of the model simulation results shown in Fig. 2 of my comment have kinetic energy of the eddies such that 'SM kinetic energy exceed that of the baroclinic component of the mean flow'. This requirement is *independent or Ri* as defined in figure 2 of my comment. The claim that the large Ri range does not satisfy this condition is simply untrue (see further discussion below).
- They have reduced the data shown to only the region where their model agrees with the simulation. The whole of their figure 4, shown in my referee report, is much less flattering to their model.
- They claim that their theory bounds $\sqrt{Ri}$ to be finite. Why chose a cutoff at $Ri < 25$ (just where it starts to disagree with simulations) instead of $Ri < 10$ or $Ri < 10000$?

Thus, I suggest that their work may be covered by Popper's 7, where 'introducing *ad hoc* some auxiliary assumption, or by re-interpreting the theory *ad hoc* in such a way that it escapes refutation' seems to be an appropriate interpretation of their arguments. Indeed, as they themselves suggest, altering their theory to improve their estimate of eddy lengthscale is possible. It is in this context that I recommended publication of this work initially, as indeed there are interesting lessons to be learned from this paper about improving submesoscale parameterizations.

Canuto and Dubovikov also suggest that I should have left out data that disagree with my theory; they are surprised that I included data which seems to support their theory over mine (the leftmost data point with errorbars near $Ri = 1.5$ in Fig. 2 of my comment). However, given the time I spent performing the simulations and the likelihood that I do not fully understand them, I do not leave out data without reason. Perhaps future readers will understand the data better than I do now. Even so, perhaps I was too generous in biasing the data toward the the limit where I believe Canuto and Dubovikov is applicable (i.e., toward the linear eddy length scale) by averaging over only data where eddy kinetic energy was 5% or more of the mean kinetic energy. They claim that their theory applies only when the eddy kinetic energy rivals the baroclinic shear kinetic energy (i.e., the questionable rationale for their $Ri < 25$ cutoff above), thus it should not matter for the applicability of their theory whether I average over times when eddy kinetic energy is 5% or greater or 20% or greater of the mean kinetic energy. By contrast, the FFH scaling *requires* nonlinear saturation of eddy kinetic energy. The following figure shows the result of an identical calculation when datapoints where eddy kinetic energy between 5% and 20% of the mean kinetic energy are left out. Apparently *all of the data that favored the Canuto and Dubovikov theory was in the weakly nonlinear regime.* Thus, my assertion that their theory does a good job when the eddies are small amplitude or quasi-linear in length scale is again confirmed. Note also that the large Ri values hardly change, which is an indication that they are not in the weakly nonlinear limit as suggested in Canuto and Dubovikov's reply.

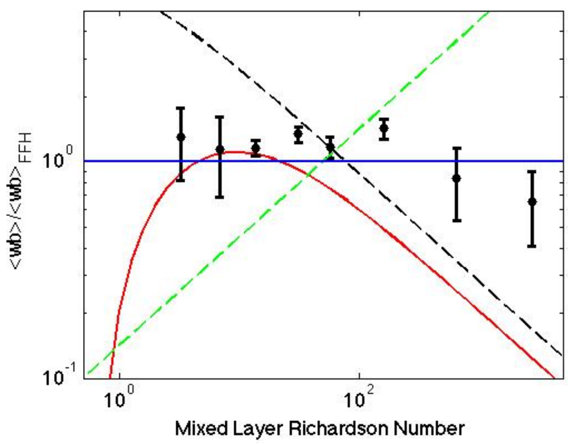

Figure 1: **Figure as in Figure 2 of the previous comment, except showing only data where eddy kinetic energy exceeded the 20% of the mean kinetic energy. Note that the one datapoint showing 'agreement' with Canuto and Dubovikov is absent.**

Indeed, the work by Canuto and Dubovikov scales at large Richardson number as the quasi-linear theory of Stone (1972) (also free of tunable parameters, but based upon linear length and time scales) does. Canuto and Dubovikov correctly note: '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'. However, as they are well aware, the simulations being discussed here share a close similarity to the Eady (1949) problem, with repercussions in the large Richardson number limit far outside of the submesoscale. Furthermore, simulations done for Boccaletti et al. (2007) showed that the Eady problem on top of stratification has the same restratification rate as the ordinary Eady problem. Thus, getting the right scaling here is more important than just for the submesoscale eddy restratification.

Canuto and Dubovikov insist that their length scale is valid in the nonlinear regime: 'It results from solving the eigenvalue problem derived from the non-linear mesoscale equations (Canuto and

Dubovikov, 2005, CD5).' So, if it is not too linear, then it is wrong. I leave this assessment to them. Granted, it is true that the linear and nonlinear lengthscales differ by only an O(1) value at first, but the nonlinear lengthscale is systematically larger and the diffusivity, restratification, and other important eddy-mean interactions depend heavily upon its value.

Finally, Canuto and Dubovikov state:

> 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.

The careful reader will note that no such blanket statement appears in my reviewer comment. What was stated, albeit without sufficient clarity, was (italics added for emphasis):

> Furthermore, new results indicate that the FFH model does work *as expected* in the presence of winds, *so long as those winds are of typical magnitude* (Capet, X., E. J. Campos, and A. M. Paiva, 2008; Mahadevan, A., A. Tandon, and R. Ferrari, 2008; see http://tinyurl.com/ylckbpo for full bibliography).

Which is completely consistent with the closing remarks of Mahadevan et al. (2010) (the use of bold-face emphasis is theirs, not mine):

> **Application of the parameterization of Fox-Kemper et al. [2008] to an along-front averaged version of the model wind wind forcing qualitatively captures the integrative effect of wind and the eddy-driven circulations on the development of stratification at a front. Restratification by the eddy-driven circulation is slowed down by the superposition of a counteracting wind-driven overturning.**

Mahadevan et al. (2010) study this issue by varying parameters so that a ratio r of the ageostrophic overturning due to nonlinear Ekman effects to the FFH-scaled eddy-induced overturning. During the vigorous eddying phase of restratification (when FFH is designed to apply), figure 5 of Mahadevan et al. (2010) shows that the fluxes change by less than roughly 50% from the FFH value, except for their strongest wind case of $0.2N/m^2$. A sustained wind of this magnitude for a duration of a month (roughly their simulation duration) is uncommon. Climatological monthly mean wind stress magnitude this high occurs less than 16% of the time (Risien and Chelton, 2008), and typically the direction of the wind within a given month varies. On the other hand, the submesoscale frontal strength used by Mahadevan et al. (2010) is common. As for the data from Capet et al. (2008b) and Capet et al. (2008a) that conflicts with the FFH scaling, it may be wind-driven or mesoscale frontogenetic (Lapeyre et al., 2006), but more work is required to *carefully* assess these issues (as Mahadevan et al., 2010, are trying to do).

3 Summary and Concluding Remarks

Canuto and Dubovikov close near the end of their reply, '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'. I do welcome their work, so long as they are willing to reconsider their length scale calculation in a 'conventional twist' and refrain from insisting that their theory is a better fit to my simulations at finite amplitude. I have repeatedly shown–through statistically-significant measures–that this claim is unfounded. A central conclusion of Fox-Kemper et al. (2008) is that the eddy length scale is *systematically larger* than predicted by linear theory due presumably to an inverse cascade (i.e., nonlinear) effect. Otherwise, our theories are surprisingly compatible given the different methods and assumptions in their derivation. Indeed, aside from the length scale, which I assert is too close to the linear result in comparison to nonlinear simulations, we agree on most aspects of the restratification process. I look forward to seeing Canuto and Dubovikov seriously consider these issues, and I will keep Canuto and Dubovikov apprised of future simulations that may confirm or refute their theory.

4 Acknowledgement

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