Response to Referee #4

We appreciate the reviewer's constructive comments, which have proved helpful in guiding our thinking. We address each of the referee's comments below and note the associated changes to the manuscript.

1. The numerical simulations seem to have been carried out seriously but nowadays, it seems really old-fashon close-science to say nothing about the code. It is now a standard practice to provide a repository of the code used to produce the numerical results. How can we convince ourselves that there is no problem in the code? In nowadays's science, reproducibility is taken seriously. Without the codes, reproducing these results is nearly impossible or at least it would demand a huge effort. If you do not want to open the code, I think you must write it explicitly.

The pseduo-spectral method has been in use since the 1970's. The only variations involve the treatment of the nonlinear terms, the times steppping, and the dealiasing, all of which are discussed in §2. We do not think it would be useful to the majority of the JFM audience to review at length this well-established numerical techique.

An important point for reproducing the results is that the unstratified cases are in power law decay as demonstrated in figures 3 and 4. The details of how this was done are analagous to the details of how a wind tunnel is physically constructed to produce isotropic homogeneous turbulence. We include a few details for the benefit of those who might want to try something similar, but if other researchers want to verify our results then they need to create isotropic homogeneous turbulence in power law decay and then apply a buoyancy force. Again, there is an analogy with laboratory experiments; researchers will normally try to reproduce published results in their own facility, not visit the original facility and run experiments there.

2. The manuscript lacks physical interpretations on what happens in the flow. When the stratification is swichted on, approximately half of the energy should be in poloidal modes, which feel the stratification directly and can behave as waves. Are waves produced? These waves should be associated with very large vertical velocity compared to Billant & Chomaz scaling. What happen to them? Then the toroidal flow should shear itself transferring energy to smaller vertical scales. What is the typical time for these evolutions? I think other analyses are necessary to study the dynamics of these flow, at least poloidal/toroidal spectra and spatio-temporal spectra.

3. The conclusions are quite weak. What do we have to retain from your study? What is rely new?

4. The last paragraph of the conclusion should be put in the Numerical methods section. I was thinking about this remark during all my first read. It would be much better to have it much before the conclusion.

5. The switch between the name-symbol Buoyancy Reynolds number-$\mathcal{R}$ and activity parameter-Gn is not clear at all. You first use Buoyancy Reynolds number-$\mathcal{R}$ for a quantity and then you say we are going to use activity parameter-Gn for the buoyancy Reynolds number-$\mathcal{R}$ of Brethouwer et al. (2007). The name-symbol Buoyancy Reynolds number-$\mathcal{R}$ has been used in many other studies after Brethouwer et al. (2007). I understand that you want to change this but then don't use Buoyancy Reynolds number-$\mathcal{R}$ for another quantity! It is very confusing!

$\mathcal{R}$ and Gn are not the same quantity as is clear from their time evolutions plotted in figure 11. As identified by Gargett et al. [1984], Gn characterizes the scale separation between the Ozmidov and Kolmogorov length scales. In contrast, $\mathcal{R}$ was introduced by Riley and de Bruyn Kops [2003] based on arguments about shear instabilities and its relevance depends on the applicability of an inertial scaling assumption, that is, an assumption that Gn is large. Hebert and de Bruyn Kops [2006] show that the two quantities differ by a scalar multiple for a set of flows of the same general type for which Gn is large. This may have encouraged Brethouwer et al. [2007] to define $\mathcal{R}$ in terms of the turbulence length scale, which makes it numerically equivalent to Gn. A major result of the current manuscript, however, is that the flow dynamics depends strongly on the magnitude of Gn and, therefore, it is essential that we consider both quantities.

We also note that in the stratified flow literature, both $\mathcal{R}$ and Gn are refered to as 'buoyancy Reynolds number' whereas in the oceanography literature that term is identified with Gn. In an attempt to distinguish between the two conceptually different quantities, we and others have recently switched to the symbol Gn and its original name 'activity parameter.' Our notation may simply add to the confusion since at least some oceanographers have asked why have changed well-established notation, but they are not familiar with the stratified turbulence literature in which the same name is used for two different concepts.

Then, the physical difference between your buoyancy Reynolds number-$\mathcal{R}$ and your activity parameter-Gn is not well discussed. Figures 11 and 12 could tell us about this difference but what do we learn from them? I think your discussion in unclear and you do not give argument why you do not consider differences in Froude number when discussing the bibliography.

Other comments

We have reviewed some of the history of $\mathcal{R}$ and Gn in response to comment 5, but we are very hesitant to go into great detail in the paper because they are simply two different quantities unless it is assumed that the integral and turbulence length scales are equal; figure 7 in **?** is one of many figures showing that this is not the case. So while $\mathcal{R}$ and Gn have been used interchangeably in some papers, making a point that they are different is likely to cause more confusion than it alleviates.

- The introduction seems to be quite comprehensive but you cite only old experimental studies about stratified turbulence. Did not you forget more recent works?

- The Numerical methodology has to be better presented. Why don't you write the equation 6.247 of Poe p(2000). You need to explain why you do this and what it implies for the decay. Same thing for the "deterministic forcing schema similar to that of Overholt & Pope (1998) (cf. ...)". Explain at least a bit what it is. Same thing for the initialisation method. In order to have a basic idea of the numerical methods, the reader needs to read four other articles.

As noted in response above to comment 1, the important point is that the initial conditions are isotropic and homogeneous and the flow is in power law decay with the decay constant given. Again, the analogy with laboratory experiments is applicable; in the famous 1971 paper by Comte-Bellot and Corrsin, data is presented to show that the flow is isotropic and homogeneous but there is minimal discussion of the details of the wind tunnel construction. We think that providing the reference as we have is an appropriate level of detail for a JFM article.

- I think many figures/captions are not of the quality standard for JFM. Looking at the figure and the caption, the reader should be able to understand without looking for a small information in the text necessary to understand the figure. Can you please also improve the figure to have no text superposed with lines or symbols. In some figures, the y limits can be changed to better see the curves.

Another reviewer has asked for additional information, such as decay rates, to be superimposed on the figures. Evidently there is a range of opinions regarding the size of the figures and how easily they can be understood without referencing the text. We have reviewed all the figures, made several of them larger,

- Figure 6: "Horizontal Froude number"... Not only horizontal.

Corrected.

- Figure 16 and its presentation: I think you should cite Brethouwer et al (2007) who present a similar figure. How did you choose the values Fh = 0.6 and Gn = 1?

Brethouwer et al. [2007] is insightful and we reference it in our paper. However, figures similar to figure 16 have been used at least as far back as talks associated with Riley and de Bruyn Kops [2003], including our APS/DFD talk in November 2002, and probably many years before that. In fact, Brethouwer et al. introduced the twist of plotting $1/Fr$ rather than Fr so that presenters of talks often pause to clarify whether the axes are in their original orientation or that of Brethouwer et al. Importantly, we plot on the Fr-Gn plane, not on the Fr-Re, plane because we conclude that Gn is the more important parameter. The horizontal lines on the plot are based on the observed changes in decays rates discussed throughout the paper.

- Figure 11: Why don't you normalized with the initial $\mathcal{R}$?

When stratification is first imposed, the assumptions supporting the relevance of $\mathcal{R}$ Riley and de Bruyn Kops [2003] do not hold. They are also do not hold at late time when $Gn < 1$. So we do not see why it is appropriate to scale by this quantity.

- You mention that "Of interest is how the dynamics of a flow, initiated at a somewhat high Froude number, are modified by the presence of stable stratification, the behaviour of the flow as the stable stratification becomes dominant, i.e., when Fr ¡ O(1), which is estimated to occur at about one buoyancy period after flow initiation". It would be good to mention that it depends on the decay laws introduced in paragraph 3.1.1.

We are not sure we understand the comment. That $Fr \sim O(1)$ after one buoyancy period is based on scaling arguments reviewed in **?**. The decay laws introduced in §3.1.1 do not include the buoyancy time scale.

- It would be easier for the reader to add the exact formula relating D to the ratio Gn/R in equation (3.2).

As stated in the sentence following (**??**), the exact formula is $(2\pi)^2\mathcal{D}$. Since $\mathcal{D}$ is a well-established quantity in the turbulence literature and the factor of $(2\pi)^2$ arises from our definition of Fr, we think that most readers will recognise (**??**) as written. We could include both, but this seems cumbersome.

References

A. Gargett, T. Osborn, and P. Nasmyth. Local isotropy and the decay of turbulence in a stratified fluid. *J. Fluid Mech.*, 144:231–280, 1984.

J. J. Riley and S. M. de Bruyn Kops. Dynamics of turbulence strongly influenced by buoyancy. *Phys. of Fluids*, 15(7):2047–2059, 2003.

D. A. Hebert and S. M. de Bruyn Kops. Predicting turbulence in flows with strong stable stratification. *Phys. Fluids*, 18(6):1–10, 2006.

G. Brethouwer, P. Billant, E. Lindborg, and J.-M. Chomaz. Scaling analysis and simulation of strongly stratified turbulent flows. *J. Fluid Mech.*, 585:343–368, 2007.