Review of the GRL manuscript "Squeeze dispersion: modulation of diapycnal mixing by diapycnal strain" by Gregory L Wagner, Glenn Flierl, Raffaele Ferrari, Gunnar Voet, Glenn S Carter, Matthew H Alford and James B Girton

This reviewer = Trevor J McDougall

This manuscript has a point to make, but it doesn't make it. Rather, there are three glaring and fundamental errors in the manuscript. These fundamental errors make this reviewer question whether the authors (of which there are many) actually understand what they have done. The paper should really be rejected, but a decision of major revision might be equally effective at getting the authors to re-consider what their paper is about, and to correct the errors. The first two of these three errors are so fundamental that I seriously hope that I have read this paper wrongly somehow, because I think that these first two issues are very fundamental and they both go to the heart of what the manuscript purports to be about, and I would much prefer if somehow the esteemed authors could be correct, and I could be wrong in my review.

**Error 1: The main message that the paper sells is wrong**

The manuscript sells the message that "Squeeze dispersion is a kinematic process that modulates and enhances oceanic diapycnal mixing when surfaces are squeezed together and stretched apart by fluctuating flow". This particular quote was taken from the first sentence of the Conclusions section, but there are many such sentences spread throughout the manuscript. Indeed this message is sold as the main conclusion of the paper. However, this message is incorrect. It is **NOT TRUE** that with a constant instantaneous diapycnal (actually isotropic) diffusivity $\kappa$, the periodic squeezing and stretching of the vertical distance between neutral density surfaces causes the average amount of diapycnal mixing to increase, and yet this is the main message that the manuscript seeks to convince the reader of. To see this, note that the instantaneous vertical flux of property $c$ is $-\kappa c_z$ and with a constant value of the isotropic turbulent diffusivity, averaging $-\kappa c_z$ over time gives simply $-\kappa \overline{c}_z$. The squeezing and stretching associated with isopycnal heaving does not amplify or diminish this flux, rather the flux is simply the average of the instantaneous flux and the same turbulent diffusivity applies to the instantaneous flux $-\kappa c_z$ as to the average flux $-\kappa \overline{c}_z$. So the main message of this paper, repeated multiple times throughout the paper, is just plain wrong.

The wrongness of this message applies when thinking about unresolved squeezing and stretching in z-coordinate models such as MOM4, MOM5, MITGCM, NEMO etc. The diapycnal diffusivity that these models should be using is in fact the one that multiples the time-average tracer gradient, $\overline{c}_z$, and the previous paragraph of this review says that this diffusivity is simply $\kappa$, not some beefed-up version of it. Nowhere in the paper is this point made. Rather, an ocean modeller, having read this manuscript, would think that they should be increasing the diapycnal diffusivity in their model because of "squeeze dispersion".

The manuscript does have a point to make; but this point is not the one that it makes over and over. The point that it should be making is that in the context of layered *thinking*, for example, in the context of layered ocean *modelling*, the diapycnal diffusivity that a layered model (such as MOM6) should be using should be the squeeze dispersion vertical diffusivity given by Eqn. (1) in their paper. This is because the unresolved vertical heaving (and hence the unresolved variations in the thickness *h* between the model's layers) is what has been averaged or smoothed during each time step of the layered ocean model. The analysis in the manuscript covers this case because the tracer *c* is known on the model interfaces but the thickness *h* is unresolved in time between successive time steps. Hence the average of the instantaneous property gradient $c_z$ necessarily involves averaging 1/*h*. This is the direct opposite situation to what pertains to a *z*-coordinate model where the vertical distance between vertical grid points is known and is unchanging, and what is unresolved during the time step are the values of the tracer at these heights. Hence what one needs to average in a height-coordinate ocean model are the values of the tracer, or, because the vertical increments of height are constant, one needs to average $c_z$.

So, in order to find the average of the unresolved flux, in one case you need to average the tracer values (and subsequently their differences in the vertical), and in the other case you need to average 1/*h*. That is, the question of whether the main message of this paper is applicable or is not applicable depends entirely on the vertical framework one is considering, that is, on the intended use of the diffusivity. It is not true that the appropriate diffusivity is always larger as Eqn. (1) of this manuscript says, but rather it depends on the use to which the diffusivity will be put; will it be used in a *z*-coordinate framework, or will it be used in a thickness-weighted layered framework. So the message of the paper needs to be substantially diluted to make it correct. As it stands now, the main message of the paper is just plain wrong.

**Error 2; Section 2 is 100% wrong**

As I understand section 2, it quantifies the vertical dispersion when there is vertical heave of isopycnals. But diapycnal mixing causes vertical dispersion *through* isopycnals, or *with respect to* isopycnals. This is how the TRE tracer experimental data are analysed; the diapycnal mixing is deduced after the adiabatic heaving of the neutral density surfaces have been eliminated from the analysis.

But, unless I am mistaken, section 2 tells us what the diffusivity would need to be in order to explain the *vertical spreading in height space*. Surely I must be mistaken in this interpretation, as it is such a silly rookie error. That is, most of the difference between the squeeze diffusivity of Eqn. (6a) and $\kappa$ is simply due to heave. This vertical spreading in *z*-space is not indicative of diapycnal mixing. It is not indicative of mixing of any kind. It is indicative of undulating adiabatic advection.

So I think the whole of section 2 is embarrassing rubbish; or on the other hand, I actually hope somehow I have missed the point of this section 2.

**Error 3; The GM90 scheme is not a parameterization of the bolus velocity**

In the penultimate paragraph of section 3 of the paper, lines 121-127, the text describes the extra velocity of the GM90 scheme as the "bolus velocity". This is not the case. The GM90 scheme leaves the momentum equations unaffected by the eddy parameterization and the resolved-scale velocity vector is the Eulerian velocity. The extra velocity of the GM90 scheme is a quasi-Stokes velocity that (like the Eulerian velocity) is non-divergent. Both the Eulerian-mean flow and the quasi-Stokes velocity of GM90 have large diapycnal components (think back to the diabatic nature of the Eulerian Deacon Cell, for example). By contrast, the bolus velocity has no diapycnal component, and it is a three-dimensionally divergent velocity.

The GM90 scheme is a parameterization of the quasi-Stokes part of the Temporal Residual Mean flow. It is true that neither the original Gent & McWilliams (1990) paper nor the subsequent Gent et al (1995) paper was correct about the interpretation of these velocity vectors. This was done carefully in the McDougall & McIntosh (2001) JPO paper, and section 10 of that paper (pages 1238-1240) proves why the extra advection of GM90 is not the bolus velocity, but rather is a parameterization of the quasi-Stokes velocity of the TRM theory.

To recap, the thickness-weighted mean velocity of thickness-weighted density coordinate averaging is the same (to third order of perturbation expansion) as the Temporal Residual Mean (TRM) velocity. This TRM velocity is the sum of the Eulerian mean and the quasi-Stokes velocities, both of which are diabatic and both of which are non-divergent. By contrast, when the thickness-weighted mean velocity is split into (a) the part that is averaged on density surfaces without thickness weighting and (b) the bolus velocity, both of these parts are adiabatic and both parts are (equal and opposite) divergent.

**An extra point that could be made**

Let us now think about the case where the diffusivity $\kappa$ varies with time. Consider a stratified fluid in the density coordinate framework, with a pair of closely-spaced isopycnals across which there is a certain velocity difference and a certain difference of locally-referenced potential density. As these isopycnals heave up and down and experience some squeezing and some stretching, let's assume that the velocity difference between the two isopycnals is independent of time. The Richardson number $Ri = N^2 \big/ \left(u_z\right)^2$ will then decrease during the squeezing occasions and will increase during the stretching occasions. This gives a positive correlation between the diffusivity and the instantaneous vertical property gradients, leading to an enhanced average diffusivity that is larger than the mean diffusivity. This enhanced average diffusivity applies when using both the z-coordinate framework and the density coordinate framework. This seems a pretty convincing straw man argument to me.

Here ends the review.