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Ocean Science
Discussions

Interactive comment on "Isoneutral control of effective diapycnal mixing in numerical ocean models with neutral rotated diffusion tensors" *by* Antoine Hochet et al.

Anonymous Referee #2

Received and published: 1 February 2018

Summary of key results

The paper estimates the effective diapycnal diffusivity due to "leakage" from the explicit isopycnal diffusion that arises from the local misalignment of five density variables commonly used in numerical ocean models and in analysis of hydrographic data from the neutral direction, namely the neutral density gamma_n of Jackett and McDougall (1997); the Lorenz reference state density rho_ref of Winters and D'Asaro (1996); and the potential densities referred to 0, 2,000 and 4,000 dbar pressure.

Using temperature and salinity fields from the WOCE climatological dataset, the authors calculate the angle between the gradient of the density fields and the neutral

direction and from this derive global mean profiles of an effective diffusivity Keff due to the projection of the (much larger) isoneutral mixing coefficient onto the dianeutral direction. For most of the density coordinates used, this angle is generally around 10-4 or less north of 40°S, and larger in the Southern Ocean; but in the case of the potential density sigma_0 it has larger values over much of the ocean interior. Integrated over all density surfaces outside the Arctic Ocean, and using two plausible choices of the isoneutral mixing coefficient Ai, this gives mean Keff profiles significantly larger than estimates made from hydrographic measurements, with the neutral density lowest and sigma_0 highest. However, if the 5% of points with the largest angles (mainly in the Southern Ocean) are removed from the calculation the effective diffusivities are substantially reduced, lying generally below 10-5 m2/s, apart from in the case of sigma_0.

Recommendation

The exposition is generally clear, logical and correct; the Introduction and Method sections being relatively straightforward to follow.

To my knowledge this is the first time such a study has been made, and I believe the results are of interest to both the observational and the modelling communities. If the changes I suggest here are incorporated I would be happy for the paper to be published.

General style

The paper is generally well structured and overall well written, although there are sections which appear to have been written by different co-authors, and the quality of English is uneven (for instance in the Abstract and the Conclusions).

The line numbering starts afresh each page, which makes referring to lines slightly more awkward for this Reviewer.

General comments

I think the results presented in the paper are interesting and novel, but it isn't clear to

me exactly what the practical recommendations of the study are. You have shown that the non-zero angle between the gradient of the density variable and the direction of diffusion give significant leakage from isoneutral to dianeutral mixing, which is generally smaller with the neutral and reference densities than with the potential densities, and you make the point that the neutral density is less useful in this context since it is non-material. You demonstrate that this leakage creates global mean effective mixing coefficients substantially larger than 10-4 m2/s below 3,000m depth, but when the largest 5% of the angles are removed from the sum the global mean Keff is reduced by an order of magnitude. I would have thought that the latter is the more interesting and useful number, since it is more typical of the ocean outside the Southern Ocean. In addition, when this pesky 5% are removed the profiles for the different density variables also get much closer together, implying that the choice of density variables has a stronger influence on the extreme values of the angle than on the more typical regime in the other 95% of the ocean. Although the discussion in the Conclusions section is relevant and valid, I feel that the qualifications above should be included in the overall conclusions.

I have my doubts about Figure 3, as I detail below.

Specific corrections:

Abstract

P1L3: Replace “impossibility to construct” with “impossibility of constructing” P1L9: Replace “isoneutral mixing” with “the isoneutral mixing coefficient” P1L10: Replace “yields values systematically” with “yield values consistently” P1L14: Replace “masses” with “mass” and insert “a” before “Lorentz”.

1. Introduction

P1L18: Need to use either “sub-grid scale” or “subgridscale” consistently throughout – not both! P1L23: This is the second occurrence of “indeed” in this paragraph: it

reads clumsily. P3L7: It would help the clarity of this section considerably if a sentence or so summarising what McDougall and Jackett mean by “fictitious mixing” were included here, as well as a clear statement of how it differs from the effective mixing discussed here. This whole paragraph, in my opinion, is too long and too sprawling in structure. I would suggest it is restructured more logically and clearly into two or three separate paragraphs. P3L20: It would be appropriate to mention here that Megann (Ocean Modelling, 2018) recently showed that the Lee et al approach gave diapycnal transformation rates in a $\frac{1}{4}^{\circ}$ÂăNEMO model that were not especially sensitive to the choice of potential density coordinate used. P4L10: “There is no question that. . .”; I would dispute that, since the APE method has generally only been used for a model that is unforced (spinning down, that is), so does not give the complete picture of the numerical mixing that occurs when the model is run in a more “normal” and useful way. P4L19: This section would be clearer if the Lorenz reference density were defined earlier in the paragraph, so that its relevance to the Lorenz reference state were more obvious. P4L23: “detph"? P4L32: Add “neutral density” before “gamma-n”.

2.1 Effective diffusivity

P5L9: The equations for theta and S evolution do not include forcing terms, and this needs to be stated explicitly here (which is stated later on for Equation 17). P5L14: Add “as” after “given”.

2.2 Reference profile

P6L25: insert “the” before “Lorenz”. P6L17: I think it would be worth stating that this definition of zr is only strictly valid where the selected density coordinate is monotonic everywhere with depth (which is not the case, for instance, with the potential density coordinates). P7L18: Insert a semicolon after Equation 18, and “this” at the start of the line.

3 Isoneutrally-controlled effective diapycnal diffusivities

P8L4: Replace “calculation” with “calculations” P8L5: It is not immediately clear why gamma-n. is not defined north of 60°N (at least in a way that the other density variables are). P9L3: Replace “sinus” with “sine”. Figure 3: Is there a mistake here? Panels A and B appear to be identical, where I would expect the values in B to be quite a bit different, since Ki is a multiplier in the expression for Keff., and presumably the former is quantitatively rather different in the two cases? P10L1-2: Replace both occurrences of “amount” with “number”. P10L2: Replace “overcome” with “dominate”. P11L7: If lines 7-11 were incorporated into the previous paragraph and a new paragraph break inserted before Line 12 the structure would be easier to follow. P11L15: Again, I am not convinced that the results for cases A and B can really be so similar, since the value chosen for the constant Ki=1,000m2s-1 used in case A is essentially arbitrary, and it would be an extraordinary coincidence if it produced results so similar to those in case B. Figure 4: The colour legend would be easier to interpret if the annotations of the log scale were in integer increments, rather than the apparently uneven ones (approximately, but not exactly, 0.9!) used here.

4 Conclusions

The discussion is relevant and interesting, but the conclusions need to be clarified, as I suggest above. As I mentioned earlier, the analysis that flows from Equation 7 is only strictly valid in the absence of surface forcing. It should therefore be noted, particularly in the discussion of Figure 4, that much of the Southern Ocean - as well as the Atlantic north of 50°N - is directly ventilated and so a good argument could be made for excluding it from the global mean in this calculation. I would guess that this might be a physically-based argument for the exclusion of the 5% of points that have large angles; perhaps coincidentally corresponding roughly to the directly ventilated regions. It would also be informative if the profiles obtained for Keff using the various density definitions were at least qualitatively compared and contrasted in this section with those estimated from observations, with those used in model mixing schemes, and also with those diagnosed for numerical mixing in models by the studies already cited here (which can

be an order of magnitude larger than the former). This comparison would put the calculated Keff values in context, and would also illuminate the importance (or not) of the 5% of extreme values for the angles in the global means.

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