Ocean Sci. Discuss., 5, S247–S252, 2009
www.ocean-sci-discuss.net/5/S247/2009/

Ocean Science
Discussions

OSD

5, S247–S252, 2009

Interactive
Comment

Interactive comment on "A new method for forming approximately neutral surfaces" *by* A. Klocker et al.

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Received and published: 10 February 2009

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Interactive Discussion

Discussion Paper

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Review of ‘A new method for forming approximately neutral surfaces’

by A. Klocker, T. J. McDougall and D. R. Jackett

10 February 2009

Summary

The authors review the difficulties in defining a ‘neutral surface’ in the ocean – that is a connected surface which everywhere lies along ‘neutral tangent planes’, upon which locally referenced potential density remains constant. Such surfaces are useful for inverse modelling, since diffusive tracer fluxes across these surfaces can only occur as a result of relatively weak diapycnal diffusion. Other surfaces, that cross neutral tangent planes, are permeable to tracer fluxes arising from (the much stronger) isopycnal diffusion.

Such ‘neutral surfaces’, strictly speaking, do not generally exist in the ocean. Because the vector field made up of the normal to neutral tangent planes has, in general, non-zero helicity, trajectories following the neutral tangent planes do not close, but follow helical paths, finishing at different depths from their initial depths when they return to their original latitude and longitude. The authors review earlier work that links the helicity to the thermobaricity, the differing variation of the thermal expansion and saline contraction coefficients of seawater with pressure.

The new material is an iterative method for refining an approximately neutral surface (e.g. constant potential density, constant neutral density) so that it becomes as neutral as possible i.e. lies as close to neutral tangent planes as possible, given the helicity.

The authors then examine the 'neutrality' of a range of surfaces approximating a neutral surface in the North Atlantic that passes through the high salinity tongue of Mediterranean Water, and thus has strong variation of temperature, salinity and depth, and consequently relatively large helicity. As well as the refined ω surface introduced here, they consider surfaces of constant neutral density γ^n, potential density, steric anomaly, orthobaric density; a recent variation of neutral density, γ^{rf}, Eden and Willebrand's γ^{EW}, and a new neutral density γ^i under development by their group. The metric that they use to judge the 'neutrality' of the surface is the spurious diapycnal density diffusivity D^f, estimated as the *isopycnal* density flux crossing the surface per unit horizontal area (because the surface does not exactly follow the neutral tangent planes) divided by the vertical density gradient.

Choosing an isopycnal diffusivity of 1000 m^2 s^{-1}, they find that only a poorly chosen potential density surface (σ_2) and orthobaric density have a spurious diapycnal diffusivity over more than 5% of the surface of greater than 10^{-5} m^2 s^{-1}, a typical observed value. However, even γ^n does have small areas with $D^f > 10^{-5}$ m^2 s^{-1}. In contrast, spurious diffusivities seldom exceed 10^{-8} m^2 s^{-1} for the refined ω-surface.

Recommendation

The new material is interesting, so the MS merits eventual publication. However I found it difficult to follow. I judge that it needs revision to clarify the material.

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Substantive Points

- p2, 2nd para of introduction. The link between the equations of state, helicity and the fuzziness of neutral surfaces is not really explained in Appendix A. Either move some of the discussion around Eqs. (9) and (11) into the introduction, or simply refer to it in the introduction
- p2, 3rd para of introduction. As well as the spurious diapycnal diffusivity because the surface does not everywhere follow neutral tangent planes, there is a further flux of tracer C across the surface arising from $\int e_a C\, dA$. Is this term significant? I guess it's difficult to evaluate e_a for a solely 2-D ω-surface, but can it be bounded by referring to typical magnitudes of $D\gamma^n/Dt$?
- I found the description of the method in section 3 hard to follow. The first paragraph should be an overview of the method, so needs to conclude by mentioning (i) how the new density surface is formed viz. by converting the Φ' into a δz using the local N^2 and (ii) that the method is then iterated to find the final surface. The more detailed discussion around Eq. (12) should go later.

  Since this is the main result of the paper, I think the matrix A should be written out, at least schematically. You are presumably reducing the 2D field $\Phi'(x, y)$ to a 1D vector, so the x-and y-gradients of Φ' will have different forms.

  If you include the constraint on the average perturbation desnity inside A in the way you describe, then it simply becomes one of the equations whose errors contribute to ϵ^2. Thus the constraint will not hold exactly.
- I suggest that Appendix A should be removed.

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Minor Points

p2, 3rd para of introduction. It seems curious to describe an MS under preparation (Jackett et al, 2009) as previous work.
p3, discussion of Eq. (4). Inverse models also require D^f for other tracers.
p4, line 4. 'between' should be 'flow across'
p4, line before Eq. (9). McDougall and Jackett (1997) refer to McDougall and Jackett (1988) for proof of Eq. (9), so replace reference.
p4, Eq (11). This is the key equation of section 2, but is hard to follow. You need to explicitly state what $\delta z N^2 g^{-1}$ represents. Also, ρ^l is not defined. It's only definition is in appendix B as 'locally referenced potential density'. Strictly speaking $\delta\rho^l$ makes sense, but ρ^l does not.
p5. last para of section 2. You mean $gN^{-2}H$, not H.
p5. last line before Eq (12). should be 'is imposed'
p5. 3rd line after Eq (12). should be 'of the theory'
p5. Why not reverse Eq. (15) and (14)? For (14) is just the direct solution of (15).
p6, 2nd to last para on left hand column. One might expect that minimizing the slope error $\mathbf{s}^2$ instead of the density gradient error should be more stable, as issues with N^2 do not arise. Is this true?
p6, 2nd to last para on left hand column. I was confused here. Do McDougall and Jackett (1988) minimize $\mathbf{s}^2$ or $\mathbf{s} \cdot \epsilon$?
p6, right column,l8. remove 'a' from 'a weighted'
p9, Eq. (21). Were S_r and θ_r chosen for this particular surface? If so, were the values chosen to minimize (21) in a least squares sense?
p9, Eq. (22). Define Φ.

p10 Appendix B, first para, line 1. Should 'horizontal' be 'isobaric'?
p10, Eqs. (B2), (B3) text should be in roman.

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p10, Eq. (B3). Define ntp in the text.
p11, last sentence of Appendix B. replace ‘is’ by ‘are’.
p12. Appendix E, 2nd para, line 5, sentence ending ‘... and west’. It would make things clearer to add ‘so that there are no zonal gradients’ to the end of the sentence.
p12, paragraph after (E2). The method only works because $\nabla_a \times \epsilon = \nabla_a \times \epsilon^{\text{init}}$, so the line structure in H on the right panels of Fig. 618 is set by the initial conditions. Presumably the whole point of this exercise is to see by how much ϵ is reduced, so you need to have a scale for the arrows to see how much smaller than 12 they are.

Figures: Clearly there has been some kind of disaster with the numbering system! Fig. 619 etc...
Fig. 618 caption should be ‘vectors’ not ‘vecors’; also ‘Shown are’ is not English syntax.

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