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Ocean Science Discussions

Interactive comment on "Geothermal heating, diapycnal mixing and the abyssal circulation" *by* J. Emile-Geay and G. Madec

Anonymous Referee #2

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Review of: Geothermal Heating, Diapycnal Mixing and the Abyssal Circulation. Submitted to Ocean Science by J. Emile-Geay and G. Madec

This paper aims to quantify the dynamic and thermodynamic effect of geothermal heating (here after GH) on the Abyssal Ocean particularly the Antarctic Bottom Water (AABW) cell of the Meridional Overturning Circulation.

On the whole I think this can be a worthwhile paper. The Authors really need to consider the structure and content of the first section.

Summary of major points that need addressing:

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Discussion Paper

A. GH uncertainty

Please attempt to quantify the uncertainty with respect to the amount of GH in the Abyss and quantify the Oceans response given these extremes. This will not only put your results in perspective, given the great level of uncertainty, but may help motivate further work on quantifying the magnitude and special distribution of the geothermal heat flux.

B. Scaling and Binning Method

You equate the magnitude of the GH flux to a mixing coefficient, and a transport twice, once using your scaling argument and once using the density binning method. Each of these three approaches are, in a way, very similar. In the first two cases you cast the strength of the GH in the heat budget of the deep ocean and scale it, using observations, in terms of a diffusivity (eqn.4) then in terms of a transport (eqn.6). Although cast in a density framework, this is effectively what the density binning method is. You quantify the formation rate of a water mass of a particular density class due to the GH term. The results shown in figure 6 are the effect of GH on the density budget cast in terms of a transport (that is why you get a similar answer to that from equation (6)). I suggest you discuss equations (4) and (6) as being similar ways of conceptualising the magnitude of the GH term to first order. This would be constructive but given the crudeness of the calculation it is not a robust result. A more robust result is gained using the density binning calculations.

C. Incomprehensible discussion of Sverdrup Balance

This section makes no sense. The manuscript aggressively opposes the work of Joyce et al. 1986 but then shows that their scaling was correct (unless you have

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made a mistake with eqn.15) and gives no explanation of the implications of your result. All Joyce et al say is that it is a small term in the vorticity equation and Sverdrup balance holds. You demonstrate that Geothermal Heating has some effect on the dynamics particularly the overturning circulation. Joyce et al. 1986 also argue that geothermal heating is important.

If Geothermal heating effects the Sverdrup balance, by how much? What effect does it have on deep circulation? This whole section should be dramatically reworked or cut out completely.

Remaining comments in order of Appearance:

1. Please be consistent with units for K_v (i.e. use either m^2 s^{-1} or cm^2 s^{-1})

2. The Authors should mention the correspondence between the actual location of Antarctic Bottom water and the spatial variations in GH. My impression is that AABW sinks into the deeper regions of the Pacific where there is little geothermal heating according to Figure 1.

3. Again what is the error $\pm$ factor of eqn.1?

4. Page 285 line 5: The statements: 'Their Surface is quite significant' and 'prolonged by continuity' are not clear. What do you mean?

5. The Authors should use ? as a symbol for potential temperature. Conservative temperature (Θ) would be better in the context of the deep ocean.

6. Page 286 line 5 'equation writes' should be 'equation is written'

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7. Page 287: Final paragraph is clumsy and final sentence (on page 288) is unnecessary.

8. Page 288 line 19: it is linear 'in' the forcing is incorrect grammatically.

9. How is the 'Bow shaped trajectory' computed? What is the reason for assuming each trajectory undergoes the same $\Delta\Theta$ How then do you calculate $\Delta\Theta$ I don't think you explain this scaling particularly well. If I am correct you are writing a heat budget for the ocean north of 45°S and deeper than 3500m. i.e.

$$\int u\Theta dA_{3500} + \int u\Theta dA_{45^\circ S} = \frac{Q_{geo}A_bottom}{\rho_0 C_p} \tag{1}$$

Up to this point you have only assumed conservation of Temperature and steady state.

I think it would be worthwhile to simply point out here that if the GH flux where to be balanced purely by advection, a circulation of 6Sv or whatever you calculate. i.e. if

$$\int u\Theta dA_{3500} + \int u\Theta dA_{45^\circ S} = \left(\overline{\Theta}_{3500} - \overline{\Theta}_{45^\circ S}\right) \tag{2}$$

The term for all ocean basins is approximately one Kelvin. I checked this against the WOCE Atlas (Gouretski and Koltermann 1996). I think this it is still consistent with your numbers but you don't have to make ambiguous comments about Lagrangian particles undergoing the same change in temperature.

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Another way of saying this is that if we are to infer the transport of AABW from temperature budgets then ignoring GH is equivalent to ignoring a term of order 6Sv.

10. Line 4, page 290 should be 'aims to reconcile' rather than 'aims at reconciling'

11. The Authors seem to really have it in for Joyce et al. 1986 and I can not see why.Joyce et al. 1986 show that the GH has a negligible influence, to first order, on the local potential vorticity balance.You show that $\overline{\rho w}$ is a larger term than the others in the integral of equation (15) by a factor of 10^4. So what gives?

12. Line 7: The word 'fertile' is inappropriate here.

I approve of the formulation of the Walin approach in density coordinates although neutral density would be more appropriate.

13. Equation (18): define α. Is it α_θ

14. Page 295 line 13: One can show

15. Better to define isopycnal 'layers' between isopycnal 'surfaces' as you tend to trip over yourself here. (i.e. 296 line 13)

There is a very clear distinction between your initial scaling approach and this one. The Density-Binning Method discusses the formation of water masses in particular density classes. Here you are correct to point out that geothermal heating causes the formation of 6Sv of dense water to lighter ones. In this sense the GH 'drives' a circulation in density space. In your initial scaling where you consider the heat budget

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between 45°S and 3500m depth you physical equations don't involve any 'driving' or inducement but merely point out that for conservation of temperature to hold there must be the equivalent of 6Sv of down gradient advection. (I.e. transport from colder to warmer waters or the equivalent in the form of mixing).

Numerical Modelling:

This section is quite well done. You use the Numerical model appropriately considering Variations to the two critical parameters: The GH flux and the vertical mixing coefficient. It is intuitive that a spatially variable heat flux would cause less warming of AABW as the heat flux is exposed to warmer waters.

One very interesting point your paper makes is that GH has a strong thermodynamic response. Although it is seldom discussed, strong artificial temperature drift is ubiquitous with ocean circulation models, particularly those forced initially by a Climatology. Did the Authors notice a difference in equilibrium time between the different model runs?

The Authors could develop a scaling to quantify the increase in steady state deep ocean temperature as a function of the geothermal heating for a given mixing rate or overturning. You could for example take the mid ocean temperature (say at 2000m) to be fixed by the source of North Atlantic and Circumpolar Deep Water.

Re-arranging eqn.4 then

$$\left|\frac{\partial \Theta}{\partial z}\right| = \left|\frac{1}{K_v}\right| \left|\frac{Q_{geo}}{\rho_0 C_p}\right| \tag{3}$$

So the vertical gradient of temperature and hence the additional deep tempera-

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ture due to GH should be inversely proportional to the effective mixing coefficient (i.e. the combined effect of mixing and advection). This is exactly what you see. If you increase the mixing of the model you mix away the additional heat added by GH. This should hold even though the shallower regions are in fact warmer.

16. Page 299 line 27 'medium' panel is incorrect panel (b) or (c) (if there where 3 panels it would be 'middle' or 'central' rather than 'medium')

17. Page 302 line 14: geothermal what?

18. Page 302 and fig.12: Surely the reason the MIX experiment does not have as large a heat flux in the Southern Ocean is because the GH flux is mixed more effectively into different water masses and therefore is more evenly distributed across each latitude band. I don't think it is clear that it is a 'nonlinear' effect.

19. Page 303 Line 9: better to say 'uncertainty' rather than 'error bar'.

20. Page 303 Paragraph 2: Better to say 'should not be neglected' than 'should no longer be neglected' as some people have considered it before.

21. Page 304 line 3: 'Allows 8230; [who?] ... to quantify'

22. Page 305 line 10: Are you limited by the resolution of your bottom boundary layer?

23. Page 305 paragraph at line 20 this statement is a bit pointless. It is just worth pointing out that we don't know how to parameterise vertical mixing and hopefully one day we will.

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24. Page 305 final paragraph: If it should be included in every model, how should it be implemented? Like STD_{Qvar}, STD_{Quni}, MIX_{Qvar}8230;? We know that the magnitude and spatial variability of vertical mixing can have leading order effects in the same way but we simply don't have the observations/understanding which is part of the reason why we don't put such things in GCMs yet. It is better to talk about what observations need to be made and what modelling experiments and developments need to be made to improve models rather than simply saying everybody should throw some GH into their model. The actual magnitude and spatial distribution if the GH flux is far from well known and you study confirms that it may be important.

25. Page 306 Line 6: I don't think GH has a gender.

26. Page 308 line 14: 'subarctic' not 'sabartic'

27. Page 310 line 1: 'A model for the global variation of depth and heat flow with lithospheric Age' should be 'A model for the global variation in Oceanic Depth and heat flow with lithospheric Age'.

28. Page 312, Table 1: This table is not very clear. I do not understand the difference between the model column and the model row?

29.Table 2: I suggest putting the overturning value and deep temperature value corresponding to each model run here (either the absolute values or deviations from STD). This might also make Table 1 clearer or you might be able to re organise the two.

30.Figure 2: Is this a specific section or zonally averaged?

31. Figure 3: Do not title this figure 'Vertical mixing coefficient' as it may be used out of context. Make sure the word equivalent or something similar is in the title.

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32. Figure 4: As above your explanation of this scaling in terms of streamlines is not clear as you do not discuss how these streamlines are defined.

Interactive comment on Ocean Sci. Discuss., 5, 281, 2008.