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Interactive comment on "Some aspects of the deep abyssal overflow between the middle and southern basins of the Caspian Sea" *by* Javad Babagoli Matikolaei et al.

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This paper investigates the overflow of dense water from the middle to the southern basins of the Caspian Sea, using an ocean model and a variety of analytical models, together with some in situ measurements. It begins and ends with a good description of the background geography and dynamics, which lead to the seasonal overflow between the basins.

The main conclusion given is the flushing time for the southern basin, which is estimated at between about 12 and 17 years.

Although there are some interesting results and ideas here, the presentation is extremely disordered and poorly evidenced. Although the authors have a model run at their disposal, they do not use the model results other than as qualitative pointers to parameters to use in the analytical models. Furthermore, the analytical models are not tested against the model results to see whether they are appropriate. Finally, the three analytical models are not clearly differentiated to explain how/where they are expected to apply, and terminology is poorly defined. In one case, this leads to a contradiction in which h is simultaneously used for the height of the interface (to determine the geostrophic current) and the thickness of the bottom layer (in the definition of PV) over a sloping bottom topography.

As a result of these problems I feel that a lot of work needs to be done before the paper meets the standards expected for Ocean Science.

The main result given, concerning flushing times, uses the model stratification to infer a flow (volume transport) over the Absheron sill, via a 1.5 layer model and some analytical approximations to the topography and interface height based on the model results. This volume transport could have been diagnosed directly from the model, with no approximations, and giving a time series as a function of interface density. Only then does it make sense to use the 1.5 layer model to test whether such a method produces a good estimate of the transport. If it does, then observational data could be used in the same way.

The other analytical models are presented with no real reason given for using them. Are they supposed to predict the path and strength of the overflow current? If so, they should be compared with the model output to see whether the prediction is good.

More generally, some bits of observational data are shown, and some bits of model data, but with virtually no comparison apart from one surface current record. It is not even clear from the description how much observational data is available: are the CTD data all one-time measurements? How many casts make up the (almost unreadable) plots in Figure 4? Equally, many of the plots seem to have randomly chosen axis labels or contour intervals which make them very difficult to interpret. In particular, Fig. 19 has many contours with tiny labels, but none match the reference contour used in the overflow transport calculation (which does not even intersect the seafloor in the January case). In a similar vein, model diagnostics are discussed on 10 different sections, A-E and I-V, with some subset of these actually shown. There is no reason given for having two different sets, and many different map regions and aspect ratios are used (in Fig. 9 the region is split into two shown side by side, for no clear reason).

I could see a route to making a good paper along the lines of the following:

1) Show observations along with comparable data from the model (the same variable and the same plot style!) to validate use of the model.

2) Diagnose the overflow from the model, calculate and discuss flushing periods.

3) Use only information available from observations to calculate an approximation to the overflow, and use the same approach in the model to validate it.

4) Investigate the flow downstream of the overflow in the model - show sections, derive parameters and compare to the analytical model.

A few particular issues:

P3 Lines 15-16 - ranges cited are not consistent with those shown in Fig. 1 - needs a comment.

Fig. 1: Why not label -5,0,5,10...deg C instead of -8... ? And the first of every other month rather than every 65 days as seem to be labelled here?

Fig. 2 (and later) - Would be better to use either sigma-theta or density minus some reference function of z. Sigma-T is not a very meaningful variable in deep water.

P4 Lines 6-7 - "there are contour gradients in the southern basin" nothing is shown from the southern basin, what is the basis for this statement?

Figure 4 - background colour is unhelpful and unnecessary. It would be helful to know the context: what is the depth here? How far from the coast are the observations? Where are the CTD casts that these contour plots are constructed from? It looks as if there must be more than the 3 marked on figure 2.

Page 6 - References should be given for the various data sources. Line 5 - what is the source of the January data used to initialise the model? Is this different from the few CTDs in figure 2? How is the initial temperature and salinity chosen where there are no observations?

Page 7 line 6 - "bottom left" should be west.

Figure 7 - needs to show the same variable (unclear if this is the case) and with the same contours for comparison.

Fig. 8 - should have the same colour scale for both, and the zero contour should be made clear.

Fig. 9 - Maps should not be split across two panels.

Page 10 - The assumptions underlying the model should be set out more clearly, and its origin (reference or derivation). It is essentially that for a freely-sliding particle on a slope under reduced gravity, but could be interpreted as a streamtube with appropriate background. "Assuming no pressure gradient" is not consistent with the earlier statement that it is geostrophic, but the g'tak(theta) term actually represents a pressure gradient term. You should also make clear that x is directed along the slope and y perpendicular to it, rather than east and north. "H is the depth of the overflow" is ambiguous - the depth of the sill? the depth of the interface between dense and less-dense water? The thickness of the overflowing layer?

Page 12 - the later discussion of E values should be included here, otherwise it is too hard to follow. What value of Ri is actually used? Saying Ri > 0.08 is not very helpful. Similarly, given the formula for r_b, a reason for choosing 2x10ˆ-5 should be given.

Page 13 - This really needs a systematic comparison with the model results - more than just saying that the model shows the flow sinks by 180-200 m (i.e. to the bottom).

Page 15-16 - the xi, psi coordinate system is described, but never used except in the axis label on Fig. 15. You seem to still be using x, y. There is a missing ")" in (5). Formatting of the equations is very strance and hard to read. Also, what assumptions go into this model? Is PV assumed constant across the flow? How can that be possible at the boundaries where layer thickness tends to zero? What input values were used to calculate the results in Figure 15? Is there any evidence that any of this is realistic? You have the actual values in the numerical model, does the analytical model predict these with any skill?

Page 17 - in (7), h is an interface height (or depth?) relative to a fixed level, whereas in (9) it is a layer thickness. These are incompatible if the flow is over topography as described.

Page 18 - line 3 "after the sill the flow depth is not changing and the entrainment effect is almost zero" - what is the justification for this statement?

Page 18 - line 5-6 "V is the downstream of the sill and D is the location at which the current is trapped by the topography" - I can't understand what this is trying to say.

Page 18 - lines 9-12 - why should the flow vanish at this boundary and not the other? This guarantees that h (whatever it is) must increase away from the boundary at which the condition is applied.

Line 15 - needs to explain that 1/beta is considered the relevant Rossby radius for this problem.

Page 19, lines 36-37 - you state that vorticity is predicted correctly and that the model confirms a prediction, but present no evidence for this.

Page 20 bottom line - I think you are also assuming that |L1|=|L2|.

Page 21 - why assume the outflow from the middle basin to be the relevant flow to calculate the flushing time? Surely the inflow is more relevant. What is the meaning of h in Figure 18?

Figure 19 - contour labels are virtually unreadable. The contour interval is not anything obvious, and the critical contour used for calculations (1008.78) should be highlighted (it is not even one of the plotted contours). In fact this contour doesn't reach the bottom anywhere in the January plot, making it impossible to identify meaningful parameters in this case.

Table 5 - I can't find a way to get values from Fig. 19 that agree with 2L values in this table. Are these actually L?

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