

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.

Anonymous Referee #2

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This manuscript discusses overflow of dense water from the Middle to the South Caspian basin. There are some previous hydrographic observations but the study is mainly via models with some guidance by the observations. A 3-D numerical model is run for several years' simulation. Analytic models, with somewhat simplified dynamics and guided by the numerical model output, are used to discuss behaviour of the flow downslope from the sill in relation to (i) amount of friction and entrainment (ii) vorticity evolution (iii) depth as a function of cross-flow coordinate, and (iv) corresponding transport and flushing times for the two basins.

These are interesting topics, although specific to the Caspian as discussed here. There

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ought to be some discussion about the circumstances in which the simplified / analytic models and approaches here would be applicable and useful in other contexts; this would make the manuscript interesting for more readers. A related concern is that there should be better motivation and synthesis of the different forms of evidence. The numerical model is compared with some rather irrelevant current observations, not very convincingly, and more favourably but only very briefly with relevant hydrography. Curiously, the numerical model is used to provide values of variables or parameters to the idealised analytic models which are used to discuss dependencies ((i) to (iv) above). One might have expected that the numerical model itself could be used; presumably it has all the dynamics (except perhaps entrainment) of the simpler models. Moreover, one might expect more comparison between the results of the numerical and simpler models. Obviously the simpler models are easier to use for exploring dependencies, but are they good enough?

The manuscript is structured reasonably and the English is generally understandable. The following “Detailed Comments” are for minor revisions but I think the issues above warrant more substantial revisions.

Page 2 Lines 2-18 are not made to relate to the Caspian. I think an alteration might be made at line 13, e.g. “. . but also in ventilation of semi-closed and closed basins, e.g. the Caspian Sea. Study . .” Lines 17-18 seem misplaced; they are not made to relate to the previous or following text.

Page 3 Line 16. “7-10” does not quite correspond with figure 1. Line 19. “16” does not correspond with lines 15-16 or figure 1.

Figure 2. Both sides would benefit from a distinct coastline. The left side should have the same latitude and longitude scales.

Page 6. Line 3. “layers” not “levels”. Line 14. I am not convinced by “are rather consistent with observations”. However, currents here are not very relevant to the sill and overflow there. Line 16 “as can be expected” and line 17 “interpolation”. The

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uncertainties due to the model grid might be estimated by comparing the variance of (model – observation) with the variance of (difference between adjacent model grid points). Figures 7 and 8 are probably better evidence that the model is working OK for the purpose of this study.

Page 7. Lines 4-5. I think this sentence “This . . Absheron.” does not add information. Lines 5-6. I think “From . . derived.” belongs at the beginning of section 3. Line 7. Refer to figure 9 as well.

Page 8 Lines 37-41. I think this belongs in section 3 before section 3.1.

Page 10. Line 5. “No pressure gradient”. There are horizontal gradients of density implying horizontal gradients of pressure. However, I think equations (1) can be OK if understood as in coordinates parallel to the slope. Equations (1). I think there might be some comparison with the model of Shapiro and Hill (1997) J Physical Oceanography, 27(11), 2381. It is very similar albeit steady-state. Line 19. In section 3.1 “re” and “rb” only appear summed as “rb+re” and a symbol for “rb+re” would be useful.

Page 13. Table 1 columns could be fitted to contents so that row 4 is all on one line. Line 14. “direction” not “horizontal”. Line 18. Word order better “. . flow is trapped after about 10 km. . .”

Figure 12. The x and y scales in the left panel differ. Ideally they should be the same but if not the caption should say that they differ.

Page 15. Lines 4-10 should somewhere state the assumption of steady flow. Equations (5), (6). In the integral exponents (of e) I think the integrand should be r/u where u has an overbar. “ r ” needs definition.

Figure 15. In the left panel, left axis, the variable should be ζ Page 16 line 39 to page 17 line 1. “. . the graph shows decreases from I to IV . .”

Page 17 Line 5. Omit first “sea”. Line 6. “similar assumptions” – but also steady which should be stated. Line 8. “No mixing could exist” - not true; no mixing is an assumption.

Line 10. “0.00002 s**-1” should be related to $0.003|U|/H$ and values of U, H.

Page 18 Line 4. “short distance between D and V” should be shown on the same figure. How are locations I, . . . V defined? Lines 5-6. “D is the location at which the current is trapped by the topography”. What about C and E for example? Line 15. To obtain R from (11) involves a value for potential vorticity Π . How is this estimated?

Page 19 line 36. “The numerical model . . . confirms this prediction.” The reader cannot infer Π quantitatively from figure 9.

Page 20. Equation (13). This form for h differs from (10). Equation (14). “)” missing after exponentials. Last line. “If we assume . . .” does not give (15) directly: $L_2 \neq L_1$. Need to say “. . . we approximate (14) by”. Then it makes sense to compare the values given for Q_v by (14) and (15) (page 21 line 5).

Page 21 lines 10-11. Better to move “are calculated” to before “below”

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