Anonymous Referee #2

Many thanks to anonymous Referee for comments on "Some aspects of the deep abyssal overflow between the middle and southern basins of the Caspian Sea" by JavadBabagoliMatikolaei et al

Our answers and further clarifications are as follows:

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.

Due to the fact that in the Caspian Sea, little research has been done on this topic, we have listed other areas of research for this section.
Regarding the study of Bidokhti and Ezam (2009), though it is not related to the Caspian Sea, we have used the results of this article in section 3-2 in our paper. As a result, we introduce this paper in the introduction.
Coming to an alternation in this regard, your comment has been implemented.

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.

Ibrayev et al., 2010 showed that the mean values for temperature in 3 basins for many years. However, we plot the temperature of each basin with hourly data collected by Modis sensor. As a result, the results of both studies should not be the same. Due to your comment, we add some more explanation for the reader to understand this difference.

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

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 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.
Two points should be taken into consideration. The first point is the model predicts the flow behavior well. The second point is the value of the velocity which is not exactly the same as that of observation data. This fact is quite natural because the model has assumptions and does not model some phenomena exactly. If a numerical model is exactly the same as the observations, then surely the performance of the model should be skeptical. According to what was mentioned, measurement data is very limited and scarce in this water basin and the velocity data is one of the best measurement data in the Caspian Sea, as this data have already been used in published paper in OS. As there are not similar ADCP data near the Absheron sill we used a numerical model in

this study. The purpose of this section is to validate the model so that we can evaluate the accuracy of the model and used its results in the analytical models.

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
Corrected

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

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.
When it comes to the model of Shapiro and Hill (1997), there is some common ground between the two papers (and this paper). However, Shapiro and Hill use stead-state assumption to solve the momentum equation. This paper solves a momentum equation for upper and lower layer because they consider the diffusion term to link the upper and lower layer dynamic. This work is very similar to Cenedese, C. J., and Whitehead, A., 2002. Cenedese, C. J., and Whitehead presented an analytical model based on two layers under steady condition with stirring diffusion.

(Cenedese, C. J.,and Whitehead, A., 2002. “A dense current flowing down a sloping bottom in a rotating fluid”, J. Phys. Oceanogr, Vol. 34, 188-203).

Apart from this, Hughes and Griffiths presented a simple convective model with effects of entrainment. They investigated roles of vertical mixing and surface buoyancy fluxes in the dynamics of the global overturning circulation . Please see: *Hughes, G. O., & Griffiths, R. W. (2006). A simple convective model of the global overturning circulation, including effects of entrainment into sinking regions. Ocean Modelling, 12(1-2), 46-79.*
Based on previous work, it means that you can solve the momentum equation analytically with some assumptions.
As a result, we should choose either steady with stirring diffusion or non-steady without diffusion to solve momentum equations, as the path of the flow was important for us, the second method was used in present work.
About different symbol for r_b+re, this’s good point, however, we tried to see separately the effect of friction and entrainment because r_b and r_e do not have the same value. For this reason, we defined two parameters to show the value of each quantity separately.

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. . .”
Corrected

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.

Because the two-axis scale is not the same. If their scale is identical, part of the flow path will not be shown. Due to your comment, some explanation has added the figure 12.

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 anoverbar. "r" needs definition. Figure 15. In the left panel, left axis, the variable should be ζ Page 16 line 39 to page.
Corrected. The formula extract from Falcini and Salusti (2015).

17 line 1. ". .the graph shows decreases from I to IV . ."
Corrected

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
Corrected.

Line 10. "0.00002 s**-1" should be related to 0.003|U|/H,and values of U, H.
Corrected. The value of each parameters from which we estimated r_b are discussed in section 3.1.1.

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?
In the new version of the paper, D and V are shown in the figure 1 to compare them with each other. The most important point in choosing these points was the distance between the two points, which is about 30-50 km. Between I and III due to the variations in the depth of the Strait, it was considered to be about 30 km, and the vorticity in these points changes more. Between III and V, depth changes are less, and therefore stretching term in vorticity (Eq. 6) equations have less effect. As a result, for this point, the distance between points is considered 50 km.
About transect E, we used the Rossby radius of defamation changes rather than transect C (see page 19, line 33-36 in main paper). Transect C is the point on the sill and can be useful to understand how much the water sinks when comparing with transect A. We discussed that in the previous section in the present text.
We assume that vorticity is the same in V~D and to estimate vorticity we use figure 15. Π is estimated based on Eqs. 5 and 6 which is extracted from Falcini and Salusti(2015).

Page 19 line 36. "The numerical model . .confirms this prediction." The reader cannot infer Π quantitatively from figure 9.
Yes. This sentence is omitted and the accuracy of the analytical and numerical model is quantitatively compared in table 3.

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: L2≠ L1Need to say ". .we approximate (14) by". Then it makes sense to compare the values given for Qv by (14) and (15) (page 21 line 5)

Your comment is quite correct and has been modified in the new revision. However, we considered a new definition for isopycnal in this part because the Strait is narrow and the shape of isopycnal lines in the sill is simpler than its shape in the southern Caspian Sea. For this reason, the terms in the h' are simpler than h also inferred than the boundary condition which is difficult to calculate the non-changing value in h profile.

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

Corrected.