Many thanks to *Dr. C. W. Hughes* 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:

Answer to Dr. C. W. Hughes

In the main paper, due to text limitation, we could not mention all our reasons for publishing this paper. Here are further reasons for presenting and using the methods in this paper:
Dr. C. W. Hughes correctly mentioned that the most important result of this paper is the flushing time. Why it is very important for us and of work?
In terms of importance, as mentioned in the paper, not much work on the deep flows and water circulations of the Caspian Sea basins, the largest water body in the world, have been done, hence, the present work may be the first attempt to concentrate on such issues which are crucial for the future fate of this sea, that have been partly addressed in the papers conclusion section.
Apart from this, there are two important points concerning the importance of this research. The first point is that there are a number of evidence which shows signs of life in the deep part of the Caspian Sea, especially in the Southern basin of Caspian Sea. For example, these signs in deep parts of this sea have presented in: Biological Features and Resources Caspian Sea, M.G. Karpinsky · D.N. Katunin · V. B. Goryunova · T.A. Shiganova, In Caspian Sea Environment, Springer, Part P (2005): 191–210).
This means that the Caspian Sea is not as Black sea which is due to lack of such ventilation is nonproductive in deep parts. But you cannot also find any research to answer this question as why the deeper part of the Caspian Sea is rather alive. The current study shows that such abyssal flow can ventilate the deeper parts of the Caspian Sea.
Another point is oil pollution due to oil exploration, especially in the Strait of Apsheron, which is underway that can affect the deep parts of the southern basin of this Sea (see Fig 1).

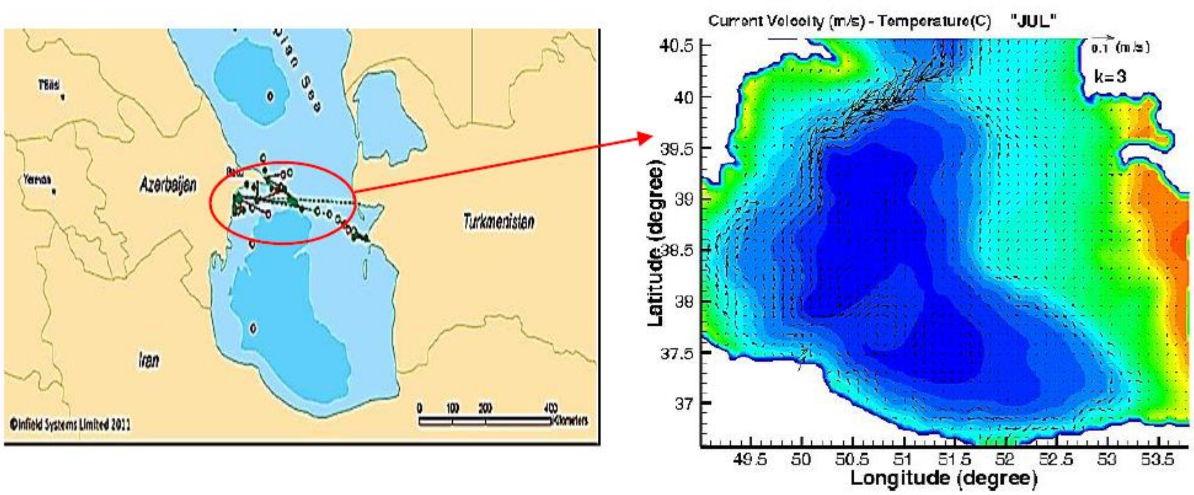
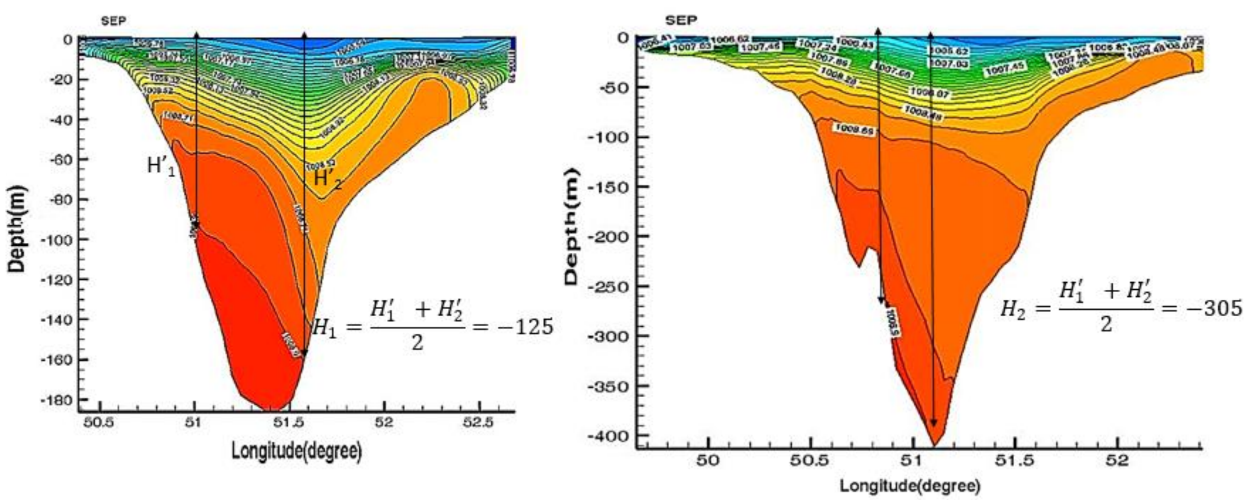


Fig 1. The location of oil wells in Apsheron strait (left) and the result of the model (right). The deep water exactly moves over the extracting points that can contaminate the deeper part.

As a result, deep flows and flushing time estimations are very important issues for evaluating the future of the Caspian Sea.

About the methods chosen in the paper, as we first began to investigate deeper flows based on existing observation data, they showed differences between the middle and southern basins in terms of density. In the first step, we tried to use analytic models with observation data. However, we found that there are not enough data to study the structure of the flow using the observation data only. We agree that the observation data for such research have no substitutions. For example, the most important points discussed are on the flow in the Strait of Apsheron and also southern basin. In these locations, the distance between the measured data is much larger than the expected current width, so we only see part of the flow and could not fully detect the current. Hence, we have to use partly some numerical model results to test analytical models.

Regarding the analytical model validation, we presented 3 analytics models in this study (Eq. 4, 13, 17). When it comes to Eq. 4, one of the best ways to test a model is to know how much water is sinking. We calculate this value 180-200 m based on Figure 11 and 18 (in the model) or Peeters et al (2000) figure 7. However, we tried to present further explanations of our approach. To deal with this, we chose transect A and C (in the paper) in September and used the density 1008.9 kg/m^{3}for calculations. The method of our calculation is shows fig 2.



$$\Delta H = H_2 - H_1 = -180 \text{ m}$$

Fig 2. The calculation of sinking based on the isopycnal lines, (left) over Absheron and (right) downstream of the Absheron sill.

In another (better) method, it can be calculated by Peeters et al (2000) measurements in figure 7 in the paper (fig. 3).

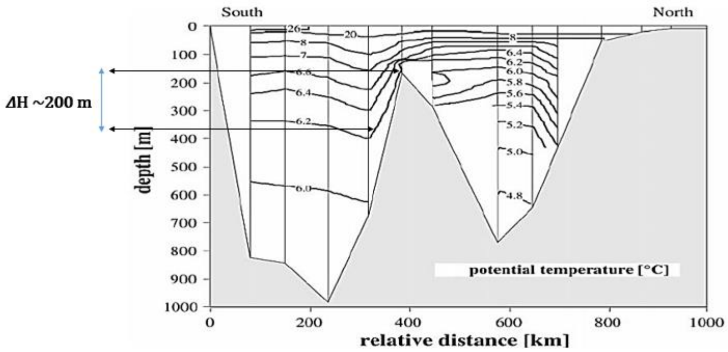


Fig 3. The calculation of sinking based on Peeters et al (2000) measurements.

Hence, the analytical model is well-matched with the results of field observations and the numerical modeling simulations. Although these figures are in the paper, in the new edition of the paper some explanations was added to the text to clarify the method.

When it comes to Eq. 13, we combine two methods to calculate the Rossby radius of deformation. We combine the method of Bidokhti and Ezam, 2009, and Falcini and Salusti, 2015 with some minor changes. Fortunately, both of paper published in Ocean Science and have also compared their model results with observation data. Due to your comment, we have added some comments on models results in the new version. For example table 1, shows the comparison between analytical model and numerical mode results. In table 1, the last column shows the direct calculation from the numerical model. Figure 4 shows our method to calculate *R* from the model.

Table 1: Boundary trapped current model parameters and values of *R*. The last column shows the result of the numerical model

	h_0(m)	h_1(m)	h_R (m)	*R* (km) Analytical	*R (km) Numerical*
NOV	120	270	400	17	18
JAN	150	440	485	15	16
MAY	110	300	400	30	34
SEP	140	430	570	25	35

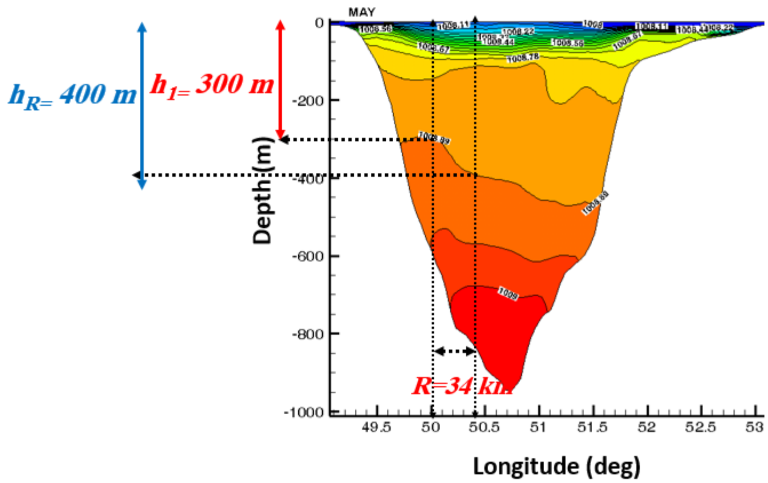


Fig. 4: The method of calculation of Rossby radius of deformation from the numerical model in the deeper part of the southern basin.

About Eq.17. This comment is very interesting and we have added some clarifications and the flow rate from the numerical results to the new version of paper. For example, you can see the comparison in table 2.

Table 2: The model boundary current parameters (1 $Sv=10^6$ m^3/s) for different months. The last column shows direct calculation from the numerical model results

	H_1 (m)	H_2(m)	$2L$ (m)	Q_V(Sv) Analytical	Q_V(Sv) Numerical
NOV	55	10	19000	0.016	0.034
JAN	145	85	32000	0.115	0.15
MAY	145	55	34000	0.146	0.17
SEP	135	45	27500	0.116	0.16

The reasons for using three analytical models are that the dynamics of the current changes when moving along the strait and then trapped along the bottom slope in the southern basin, so we used three analytical models to describe the dynamics of different stages the flow. To explain more about the presentation of three models, as in the Iranian seas (Strait of Hormuz and Apsheron sill) there isn't any ADCP data to understand some important parameters of outflow in basins, we used these models. For example, in the Hormuz Strait, CTD data could be the only data to specify the outflow from the Persian Gulf that are non-extant appropriately. Hence, thanks to this kind of model that can used to calculate many parameters of outflow without using ADCP data, for example, the volume of outflow due to using the Eq. 15.

About presentation, we agree with your opinion when it comes to using some parameter such as h in different meanings. We have faced with some problems to present the model because we have to define many parameters due to using three analytical models. In the new version, we have changed some parameters and added some explanation about our methods.

About observation data, we used two kinds of observation data, CTD and ADCP, in this paper. The CTD data are obtained from UNESCO Atomic Energy International Agency, for summer

1996. This data is recorded from 25 August to 9 September. The data are collected at 27 stations using an Investigative ship in this project namely Hajef. ADCP recorded by RCM9 current meters (at the ADCP station) at 3 depths on a mooring, near the surface, 50 m, and 200 m near the Iranian coast. We used this data to validate the numerical model. This data is the only exiting one that are collected in the southern Caspian Sea. For the method of these data collection, please see:

Ghaffari, P. &Chegini, V. (2010). Acoustic Doppler Current Profiler observations in the southern Caspian Sea: shelf currents and flow field off Feridoonkenar Bay, Iran. *Ocean Science*, *6*(3), 737.

Other comments of ***Dr. C. W. Hughes***

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

Although we accept your suggestion, according to what was mentioned, observational data are very limited. In some places, we compared exactly the observation data with numerical model, for instance the figure 5 and 7. We confirm that if there were sufficient data, there would be no reason to use the numerical model. We emphasize that only observation data (CTD) is used to find the existence of the gravity driven flow between the middle and the southern Caspian, as Peeters et al (2000) measurements also indicate.

2) Diagnose the overflow from the model, calculate and discuss flushing periods.
In the new version, the flushing time is calculated directly from the numerical model as well. The calculation shows that the flushing time is about 6-7(middle basin) and 13-14 (southern basin) years for direct calculation from the numerical model results based on z=0.

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.
Above discussion may be enough for this question.

4) Investigate the flow downstream of the overflow in the model - show sections, derive Parameters and compare to the analytical model.
We add this part in the new version of the paper. We compared the numerical model results with the analytical model ones based on your interesting comments.

A few particular issues:
P3 Lines 15-16 - ranges cited are not consistent with those shown in Fig. 1 - needs a Comment.

The difference comes from the years that have been chosen in the research and are not the same as fot Ibrayev et al., 2010, and also, we used data every 6 hours, but probably they used monthly average data. Some more sentences are added to the paper for clarification.

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?

There is not any special reason for temperature axes. The output itself is in the Excel format and the changes are done in the new version. Regarding the months, if every month is given, the axis will be crowded and could not be readable. In this axis, since the series has been for two years, the year should also be given.

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.

We accept your comment although density should only be used in figure 3 due to a depth of basin. In figure 4, due to the depth of water on the Strait (180-200 m), we can use Sigma-T. In another part of the paper, the density is used for the calculation. Hence figure 3 has been changed in new version.

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.

It means that figure 4 shows the outflow due to the slope in the sigma-T contour, and figure 3 expresses the density differences between the middle and southern Caspian Sea basins. However, your comment is right and in the new version, it is corrected because our observation data are for the Absheron strait.

Figure 4 - background color is unhelpful and unnecessary. It would be helpful 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.

Regarding your opinion, we redefined figure 4. But we think that the figure in the paper is better than the new figure. This is not a specific problem and can be replaced in the final version.
The depth of water in this area is about 180 to 200 meters. The distance of Azerbaijan coast is about 80 km from this area. In figure 2, we have identified the geographic location of observational data (see transect B, Fig.2). This information is added to the text in the new version.

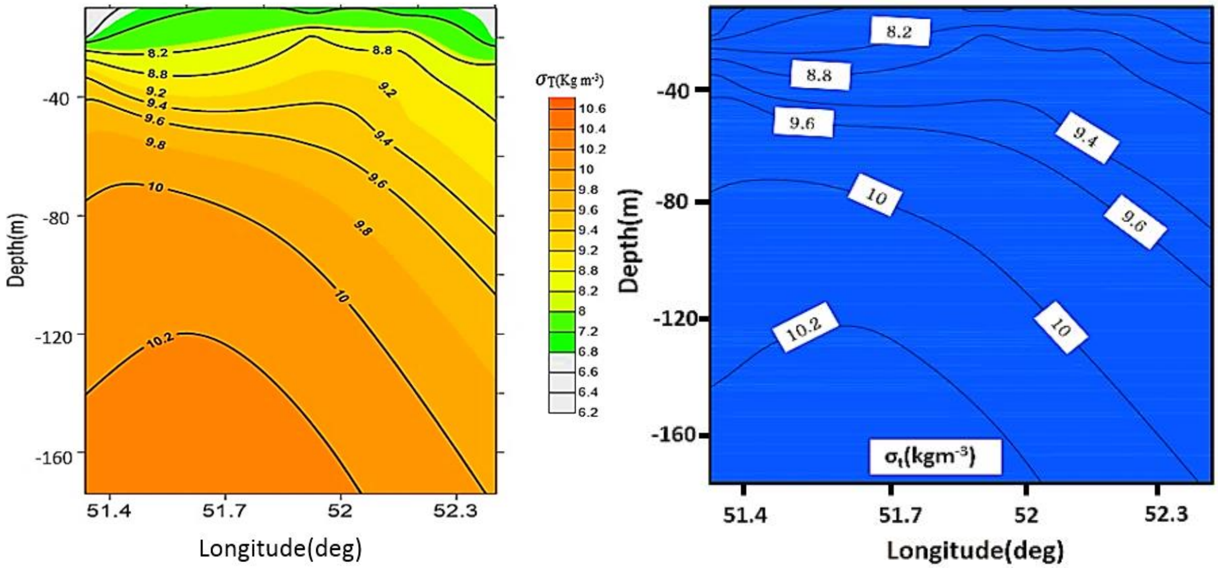


Fig. 4: different background color for Fig.4 in the paper.

Page 6 - References should be given for the various data sources. Line 5 - what is the source of the January data used to initialize 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?

Yes. It is corrected. It means that the model was initialized for winter (January) using monthly mean temperature and salinity climatology obtained from Kara et al (2010).

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

Corrected

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

Although your suggestion is quite good, we cannot compare them because the numerical model (present work) and Peeters et al (2000) are not for the same years. Comparison is meaningful when the two quantities are the same. We emphasized this in the revised paper in that the overflow occurs in the Caspian Sea on the Absheron sill. However, we tried to apply your opinion in the new version.

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

Corrected

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

We tried this method but the details of the current are not displayed. Typically, according to your comment, in Figure 5 we have a sample.

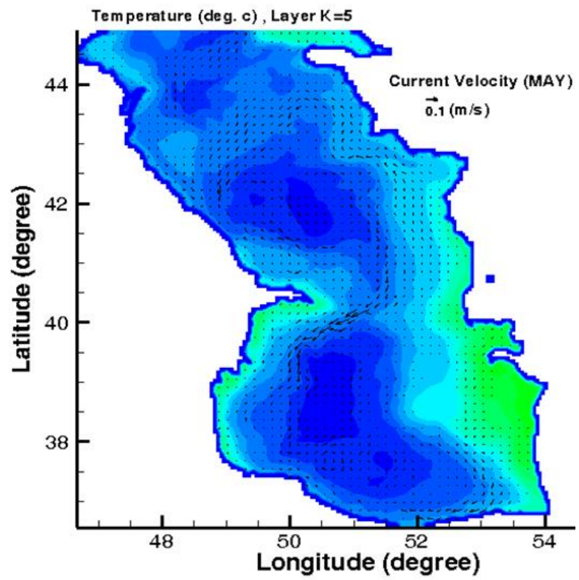


Fig. 5: another style of display Fig.9 (in the paper).

Page 10 - The assumptions underlying the model should be set out more clearly, and its origin (reference or derivation). It is essential that for a freely-sliding particle on a slope under reduced gravity, but could be interpreted as a stream tube 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?

We used the streamtube model in the section 3-2. In this part, the path of the flow is more important for us. We are going to examine the path of particle motion based on the Lagrangian view. We wrote the momentum equation for particles and the most important assumption is that we considered entrainment effect as well as friction. Eq. 1 is right because we consider the coordinates parallel to slope.

H is the thickness of the overflowing layer.

Origin of the overflow is from the top of the sill between the two basins. About stream tube, we use this method in section 3.2 to study of vorticity when the flow moves over the strait. More clarifications are also given in the new version.

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.

To calculate Richardson, we use $Ri = \frac{g'H}{U^2} cos\theta$. Based on our result, U which is 0.1 to 0.2 m/s, g'=0.00222-0.00251m/s^2,H=50-70 m, tanθ=0.02. $Ri = \frac{0.00251*50}{0.2\times0.2} 0.99$~ 3.1. Based on Ashida and Egashira (1997)E*=0.0015Ri^{-1}. Hence, E*~0.0005 and re~E/H=0.0005/50= 1×10^{-5}.

According to the Cheng et al, 1999, Cd~2-6×10^{-3} the drag coefficient is not a constant value. Hence, we consider two value for Cd~3×10^{-3} and 5×10^{-3}. As a result of $r_b=C_du/H=0.005*0.2/50=2\times10^{-5}$.This calculation was added to the main text.

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). This was discussed above. Based on your opinion, some extra explanation was added to the new version of the paper.

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 wereused 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?

Falcini and Salusti (2015) presented an analytic model for vorticity and PV (ξ, ψ) coordinate system. We use this system to estimate the relative vorticity when moving on the sill. After the sill, dynamics of the flow change and appear as a trapped flow. Hence, in this paper, we used (ξ, ψ) coordinate system only to calculate relative vorticity based on Falcini and Salusti method. Potential vorticity is assumed constant along the flow until the slope of the isopycnal is zero, hence we consider this assumption. Figure 15 is plotted based on the Eq. 5. The input values are some quantities such as u, $\partial h/\partial x$…. these quantities are extracted from the numerical model results.The first step is an estimation of Π moving on the Apsheron sill. On the other hand, Bidokhti and Ezam considerd that the potential vorticity was conserved, when moving from the sill, as they ignore friction and entrainment. They presented their analytical model to calculate Rossby radius of deformation when the flow was trapped on the topography. We try to develop this theory by considering the bottom friction and entrainment. The problem comes from this reality that the potential vorticity is not conserved based on our assumption. As a result, we estimated the changes of vorticity on the strait based on Eq.5 from Falcini and Salusti (2015). In terms of its accuracy, we add this part to the new version and also discussed above.

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.

In this part, we tried to use the previous paper published in the ocean science (Bidokhti and Ezam, 2008; Falcini and Salusti, 2015). In this model we define two layers flow together with boundary conditions. This method is better than using the bottom as a reference because the depth from bottom is not equal along the isopycnal.
In this model the two layers move together and we can define boundary conditions (BC) better when considering the surface BC.
In Eq. 7 and 9, the definition of h are the same in both of Eqs.

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?

This indicates that stretching term is not important in vorticity because for this kind of flow the depth changes that can change vorticity. The entrainment effect is almost zero because Ri~ 50 and based on table 1, this effect can be ignored.

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.
Yes. It is not clear and we added more clarifications in the new version so the reader can understand the location of V and D simultaneously.

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.
The flow vanishes at the boundary because the slope of isopycnal is zero. As a result, we consider this assumption at the boundary. Thanks to using numerical model results, the velocity field also confirms this theory when the slope of isopycnal is zero, the velocity is also zero. Hence, it is a guarantee for us to check our analytical method results.

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

Corrected.
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.

This was discussed above.

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

Yes. However, we defined new parameter L in the obtained new formula, which means that this definition $L = \frac{|L_1|+|L_2|}{2}$ is enough and not contrasted with|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?
That's a good question. It depends on how observer look the phenomena when we look the flow in the middle basin is outflow (because getting out from middle basin) and you consider southern basin which indicates that the flow is inflow. It shows that you and we describe one phenomenon in a different view.
About *h*, in the new version, we use h' because it is different from h. h' is the distance of isopycnal from bottom in L_1 and L_2.
It should also be mentioned that the densification due to evaporation and freezing mainly occur in the northern shallow basin attached to the middle basin, hence the deep overflow from the middle basin to the southern one is often usually used as in case of semi-enclosed sea as Mediterranean sea.

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.

About the choice of 1008.78 kg/m^3, this contour has been selected with a detailed study for all months. The major problem is the position of the contour which changes for different months. If we consider the contour 1008.9, it is good for some months like January, but it is not useful for November because the maximum contour is 1008.8 for this month. I agree with you about January, we can choose different counters for each month to solve this problem. However, this method cannot be useful to compare all month in terms of Q_v. For this reason, we try to use the same method for calculation, although we accept your opinion. This create an error in limited months such as January. For this reason, we show how the error can occurs using this assumption (Fig.6).

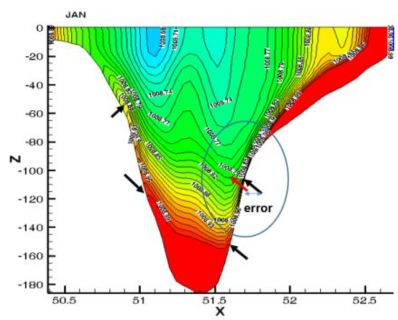


Fig. 6: The black arrows shows that the points which is used to calculate Q_v. Near the bottom is ok without any errors. However, 1008.78 dose not reach the bottom but we assume that it reachs the bottom.

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?

The first step is the calculation of L_1 and L_2. Then L=L1+L2/2.To examine the accuracy of the calculation of L, the length of the strait is between 10 to 59 km in deep parts. Based on table 4, 2L is 19 to 34 km. It is rational because 2L is smaller than 10-59 km. 2L=19 km is for November because the contour 1008.76 is found more suitable for calculation of L.