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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.

Anonymous Referee #1

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Although perhaps not pure "Ocean" science, the subject of deep overflow between two basins of the Caspian Sea ought to be of sufficient interest to the readers of Ocean Science to merit publication. Unfortunately, I do not find that this manuscript presents significant new information on that subject and it seems to contain highly questionable results, as will be elaborated below. I therefore cannot recommend publication of the manuscript.

1. Not being familiar with the Caspian Sea, one of my first questions was: "What is the evidence for an overflow?" I did not find a very satisfactory answer in the manuscript. The introduction gives some general information on the Caspian Sea, but I missed information on what is known about an overflow. Has it been discussed in previous

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scientific publications and what is the evidence? I only found a reference to Gunduz and Özsoy (2014), in which an overflow is mentioned, but there, they claim “limited evidence”. The Introduction ought to state clearly, whether this overflow has been described in the literature and its main features. If its not been well described previously and its evidence is limited, then this should also be clearly stated in the Introduction. If not clearly identified in previous studies, observational evidence for an overflow ought to be presented, but the only observational evidence for an overflow that I found in the manuscript was in Sect. 2.1, where CTD profiles from two stations were compared and it is stated that “Due to the density difference between the middle and southern basins, dense current crosses the Absheron sill”. This is indeed a possibility, but certainly not the only one. And, even if there is an overflow, how deep does it go? Is it persistent or intermittent? etc.

2. Thus, the only evidence for an overflow, that I found in the manuscript, is from modeling and the manuscript presents results from three separate models, one numerical and two analytical models. I have no experience with the COHERENCE numerical model, but offhand the results presented seem interesting. I miss, however, information on some key questions: How well does the model represent the (apparently scarce) observational evidence, such as the CTD profiles ? How sensitive is it to mixing parameters ? Does it show overflow ? Especially the last question would be very relevant. From Figure 4, the temperature close to the bottom of the southern basin is not equal to that at the sill. Does this mean that the overflow is modified by entrainment on route ? Or that it does not go all the way to the bottom ? Or is there a well-defined overflow in the model and what are its properties ? The manuscript refers to Figure 4 and states that: “...the overflow over the Absheron sill and in the north western boundary of the southern basin are clearly observed”, but I do not see this clearly from the figure. Yes, there is strong flow, but is it descending and is the descent density-driven ? I miss this information in the text. It appears that the numerical model is mainly used to provide parameters for the analytical models.

3. The first analytical model (Sect. 3.1) looks to be a very simple model. I have not checked the equations and will assume that they are valid, but I find the definitions of the model badly presented. The text says that: "local coordinates xÊź is along the flow", but if that were the case, you would expect the y-velocity v' to be identically zero. From the equations, I assume that the bottom is a flat plane tilting an angle θ from the horizontal and that the x' direction is downslope. But, how well can that represent the conditions at the Strait of Absheron. In Figure 9, the model tracks a water particle moving 30 km downslope, which must imply a deepening of 30000x0.02 = 600 m. On its way, it has to pass transect C, where the dense water according to Figure 6 is not on a plane, but confined within a fairly narrow channel. Also, the model seems internally inconsistent. In Figure 1, I have plotted the tracks of two water particles starting downslope at the same time, but with one starting 5 km displaced relative to the other (in y-direction). The two particle tracks cross. Apparently, this model describes an individual water particle, not affected by the motion of the neighbouring water, and I do not see any reason that it should reflect reality.

4. The second analytical model also seems very questionable. It assumes conservation of potential vorticity, but it includes friction and PV-conservation usually assumes inviscid flow. The text says: "... the potential vorticity is conserved, because of topography in the South Caspian", which is not clear to me. But, there are other problems with the model. From the definition of h, Eq. (6) is for the upper layer, but Eqs. (7) and (8) must be for the lower layer. How can they be combined ? Like in the previous model, friction is assumed to depend linearly on velocity, which is not very realistic and there is no justification for the chosen values for r.

5. The results section ends with a calculation of flushing time, but again I have difficulties with understanding it. Where does the expression for h in Eq. (13) come from ? It is not consistent with Eq. (10). When it is stated that L1 = L2 = L, I assume that there is a minus sign missing, but even so, these points should be where the interface hits bottom on both sides of the channel (Figure 12). How can you assume that they

are symmetric around the center of the channel ? There is a rich literature on analytical overflow models, even with parabolic bottom shape (e.g., Borenäs and Lundberg, 1988). I would suggest using one of those for this calculation.

6. There are a number of details that should be corrected: small figures, small figure legends, using rho instead of sigma in some figures (is it corrected for pressure ?), using the term “Rossby length” to represent the actual width of the flow, the reference to Gunduz and Özsoy (2014) in the reference list is wrong.

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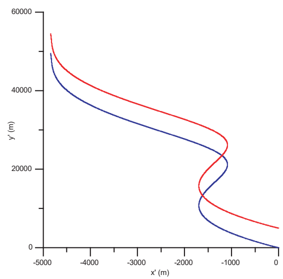

Fig. 1. Figure 1. Tracks of two water particles (y' versus x'), starting at time t=0 with the red track displaced 5 km from the blue using Eq. (3) for NOV in Table 1 and r=0.00001 s-1.

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