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Ocean Science Discussions

Interactive comment on "Numerical simulations of spreading of the Persian Gulf outflow into the Oman Sea" *by* M. Ezam et al.

Anonymous Referee #2

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The manuscript "Numerical simulations of spreading of the Persian Gulf outflow into the Oman Sea" by Ezam et al. describes results from a numerical simulation of the exchange between the Persian Gulf and the Oman Sea in two different times of the year. Although the topic is interesting and still not well covered in published works and the numerical model employed is adequate for the purpose, the work suffers from a few important deficiencies that prevent it, in my opinion, to be accepted. According to my judgment, the numerical experiment was not adequately designed, thus compromising the conclusions.

The Persian Gulf is a very shallow marginal sea in which heat and freshwater fluxes are of leading importance for the dense water formation in the basin. In the absence of those, the dense water supply to the model domain is maintained by the western open

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boundary condition. The authors should then be more concerned with the construction of this lateral forcing and they should show in the manuscript the observational data used to build the boundary condition (and not only the interpolated fields). Is the model relaxed (or nudged) at the boundary to the shown temperature and salinity distributions? And what about the eastern and southern boundaries? It is only mentioned that the hydrographic data derive from the ROPME 1992 measurements but I could not find a publication with the westernmost CTD section (section A in the map of Bidokhti and Ezam, 2009) in order to compare with the distribution after interpolation. I believe (as acknowledged by the authors) that not enough sampling exists to constrain the temperature and salinity distribution at 54W. Furthermore, the authors only briefly mention the velocity boundary condition and they do not show the magnitude of dense transport at the western boundary or at the Strait of Hormuz. So, since all of the buoyancy forcing is projected into the western boundary, more details have to be given. I would, however, suggest the authors to include the whole Gulf and the important exchanges at the surface (as they actually intend to do, as stated in the conclusions).

My second concern relates to the duration of the integration. This is not mentioned at all in the text and I wonder if the authors did bring the model into some level of equilibrium for each of the two times of the year. I could not understand if a continuous forcing run was performed from February to May or two 1-month-long runs... The authors have to be clear on this matter in order to build some confidence on the obtained results.

Regarding the results downstream of the Strait of Hormuz, it seems that the overflow plume goes unrealistically too deep in February (there is absolutely no observational indication of an outflow plume at 500 m depth in the Oman Sea). I can only conclude that the overflow's density is too high due to: wrong inflow properties at the western boundary, insufficient mixing to erode the density signal when the plume is descending or lack of surface forcing. The problem could also be related, for instance, to wrong bottom drag or to unrealistic ambient water in the Oman Sea, leading to inadequate entrainment. If the authors do plan to keep their experiments idealized, then a few

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sensitivity experiments should be conducted to support their simplifications.

Finally, I suggest the authors consult an English technical editor before submission in order not to compromise the reading and understanding of their scientific results.

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