

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

Anonymous Referee #1

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The paper "Numerical simulations of spreading of the Persian Gulf outflow into the Oman Sea" by M. Ezam et al. proposes an interesting topic. Unfortunately, some basic hypotheses and the presentation of the paper suffer important weaknesses. These problems lead to a negative recommendation for publication of the paper, in its present form.

In brief

1) As the authors state, the neglect of tidal effects and above all, of thermohaline forcing at the surface, is a severe limitation for realistic model simulations of this area. They observe that the surface layer characteristics are not well reproduced by their model (and sometimes quite below the surface). But the major problem is that the Persian-Oman gulf exchange is driven by a pressure difference between the two ends of the

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Straits of Hormuz. This difference is due to the water density. This area is shallow. Therefore, inaccuracies on surface water characteristics affect density, which modifies pressure, which affects the exchange, and finally the outflow.

2) Bottom of page 3075, "these assumptions may have minor influences in our study". Which facts support this conclusion ? On the contrary, model result comparison with observations show that, at least, the thermohaline forcing at the surface should be included.

3) Observations in the Oman Gulf do not span the whole year, but they do not evidence a deep outflow. Both the Bower et al papers and the Pous et al papers mention a PG outflow at 250m depth. 300m depth may be acceptable, but there is no report of a 500m deep outflow.

4) pages 3073 and 3074, the authors give a list of theoretical results that they do not apply to their results (for most of them). What is the point of such a list?

5) Unfortunately, the paper also often suffers from a poor English which renders the reading very difficult. Also, the structure of the paper should be better organized since it appears that several data interpretations are repeated in the text (where the outflow lies, why the thermohaline characteristics are different in February from May ... etc). At times, indications on where some features lie are not clear (this concerns eddies). Also, if possible, the authors should call "eddy" (sub)mesoscale structures, and "gyres" regional features. The paper should be proof-read before resubmission.

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