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Interactive comment on "The circulation of the Persian Gulf: a numerical study" *by* J. Kämpf and M. Sadrinasab

E. de Goede (Referee)

Erik.deGoede@wldelft.nl

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This paper contains a very detailed description of the physical system and the applied hydrodynamic modelling for the Persian Gulf. However, for the reader it is difficult to separate the main issues from all details. For example, the main goals are described in one paragraph on page 133 (see "This paper focuses ... lacking?"), which is the fifth page of the manuscript and is "hidden" between a lot of details of the description of the physics in the Persian Gulf.

In general, the authors show an impressive knowledge in the behaviour of the physical system in the Persian Gulf. However, a weaker point is the hydrodynamic modelling. For example:

- 5 (!) sigma layers are used. I would say that 10 (or perhaps 20) layers is nowa-

days the standard for modelling stratified conditions. I have therefore serious doubts whether 5 layers are sufficient (for a region with locally depths of 140 m).

- The authors claim that their model captures mesoscale instabilities ($\sim$20 km), see page 133, while their model resolution is about 7 km. Three grid points are certainly not enough to model phenomena on a scale of 20 km.
- A problem with the upwind advection scheme is shown (see Figure 4). Since the model resolution is already rather fine (mesh size of about 7 km), it is expected that even a first-order upwind scheme should produce relatively accurate results.

All together, there are some doubts about the performance (c.q. accuracy) of the Persian Gulf model. Their conclusion ("on the basis of our simulations previous suggestions ... *need to be revised*") is in my opinion too strong. Furthermore, the authors also suggest future improvements (e.g. spatially varying meteorological forcing). So, I am not fully convinced yet.

In Chapters 3 (results and discussion) and 4 (conclusions) a lot of conclusions are drawn. However, most of them can not be verified by the reader. Furthermore, the most important figures (5 and 7), with the Box 3 and Box 7 results, are not very clear. So, in general, it is rather difficult for the reader to verify some of the conclusions.

Summarizing, the manuscript is certainly worth publishing. It is evident that the authors have done a lot of work: detailed analysis of the working of the physical system, detailed analysis of measurements, detailed description of the performance of their hydrodynamic model, etc. However, I would propose a revision of the manuscript in order to:

- separate the main goals from the many details in the manuscript;
- elaborate more on the issues mentioned in paragraph 2.6 (e.g. which parameters with have a large impact on the model results and which not?).

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- better illustrate the performance of their model (additional/other figures with a comparison of model results with measurements). Is it e.g. possible to make colour figures for the field observations in Figures 5 and 7?
- Somewhat "weaken" their conclusions about the performance of their hydrodynamic model, because there are some doubts (see above).

The quality if the description of the physical system in the Persian Gulf is excellent (although perhaps a bit too detailed), the quality of the hydrodynamic modeling seems to be average (in particular, due to the 5 layers in vertical direction).

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