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Discussions

Interactive comment on "Seasonal variability of the Caspian Sea three-dimensional circulation, sea level and air-sea interaction" *by* R. A. Ibrayev et al.

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The paper of R.A. Ibraev et al. is subjected to the simulation of the Caspian Sea hydro thermodynamics with realistic atmospheric forcing and comparison of the model result with available observations. Rather sophisticated model is developed by authors to carry out simulations. It is based on the circulation model with free surface coupled with air-sea interaction module which allows adjust atmospheric fluxes to the sea state and sea-ice thermodynamics. Atmospheric forcing is slightly corrected ERA-15 reanalysis for 1982 which was selected as the annual sea level increase was smallest during that year according to observations. Simulations presented in the paper under the review

to my knowledge are the best for the Caspian Sea.

The circulation model is well designed and clearly outlined. However models of sea ice and air-sea interaction are described very briefly with reference to other publications for more details. Probably it is better to provide broader description of both sub-models in the text of the paper and emphasis specific problems that were solved.

I can also recommend authors to describe better how simulations were fulfilled. It is not clear how non-periodic boundary conditions (atmospheric forcing, river runoff) were used to reproduce 1982 seasonal cycle. It is not clear why the net heat flux during the last year of integration is almost equal zero? What about the net fresh water budget?

The paper presents general description of the seasonal cycle of currents, temperature, salinity, sea ice and sea level. Special attention is paid to the seasonal variability of air-sea fluxes. It follows form the paper that the agreement between model results and that what is known from observations or another model studies is quite reasonable. This is very important characterization of the model quality if 3D state of the basin is formed only by air-sea fluxes and river runoff. However the model starts from climatic state of November and it is not clear from the text how sensitive simulation results to the initial conditions.

It is shown by authors that the Caspian Sea is a very delicate basin rather sensitive to input parameters. Sensitivity study presented in the paper permits to evaluate qualitatively possible distortion of the simulation results due to uncertainties in the fluxes on the sea surface, river runoff, etc.

At the same time it seems for me that it is possible to present broader comparison of simulations and observations. I do not understand why nothing was done to compare simulated temperature and salinity fields with climatic data base or at least with simulations of Ibraev et al. (2001). Maps of SST distribution can be compared with SST maps retrieved from the space IR measurements to show how the model reproduces spatial SST distributions. Such comparison is useful to show the quality of radiation

and heat fluxes on the air-sea interface.

Probably there is a reason to extend analysis of the sea level evolution during a year. Authors mention on page 1917 that the sea level oscillations during a year reflect annual variability of the net water budget. Therefore a major part of the signal shown on Fig. 19 can be obtained from a simple water balance equation $S \cdot \frac{d\bar{\varsigma}}{dt} = W$ (S - sea surface area, $\bar{\varsigma}$- sea level averaged over the basin area and W - net water budget. This means that the comparison of simulated and observed sea level variability on Fig. 19 characterize mainly the quality of the river runoff, precipitation and evaporation data. Only deviation of the sea level evolution on every of four sites from the sea level evaluated by the water balance equation is important to show circulation model skill. I recommend authors to provide such comparison with observations. Other case Fig. 19 is just misleading.

The paper contains a set of conclusions which clarify the Caspian Sea hydrothermodynamics (fronts, gyres and their evolution, spatial variability of temperature). The most interesting of them is an explanation of a warm water tongue along the eastern coast which appears due to a warm water transport to the north by sub-surface current though surface currents are directed often to the south. Those conclusions are of great interest and prove the expediency of the paper publication.

However an advantage of numerical simulations if to compare with observations consists in the possibility provide full analysis of the most interesting phenomena. Validation of some model results by observations justifies a need to extend interpretation of simulations. Therefore it is pity that authors restrict themselves mainly by the analysis of processes manifested on the sea surface or close to surface. I think that it is interesting to pay attention to the deep sea ventilation and water mass formation in the Caspian basin. Usually deep basins have restricted areas of convection penetrated to the bottom. Sometimes this process is intermittent with time. I believe that it is important to clarify what is the process of deep water formation in the Caspian Sea according to the presented model simulations in spite of the sort time of integration.

It is also interesting to show peculiarities of the meridional heat and salt fluxes in the basin with emphasis of relative importance of vertical overturning and lateral transport. I am not sure that authors are able to include proposed aspects to this paper but may be they will consider them later.

I think that the paper addresses scientific questions concerning to the seasonal cycle of the Caspian Sea fields. In spite regional significance of the paper it is relevant to the scope of OS to my understanding.

Introduction contains brief review of the current state of the Caspian Sea investigations and permits to distinguish new original contributions by authors.

The title clearly reflects the contents of the paper. May be according to the weight of different sections of the paper it is possible slightly correct it like this: “Air sea interaction, sea level and circulation. . . ”

The abstract provide a concise and complete summary. The overall presentation is well structured and clear in spite it is too brief sometimes. References are good enough. The language is also good for me but I am not an expert in English

Some particular remarks:

1.Page 1917 lines 11-13. The sentence should be edited: One third of entity has the same order of magnitude as an entity itself! Also the sentence sates in fact that the sea level grows continuously as evaporation compensates river runoff and precipitation is big enough.

2. Page 1917 last line: fluxes across the SEA surface, not ocean surface

3. Page 1918. I do not understand the meaning of the last line.

4. Page 1921 equation 10. Density $\rho_f$never defined.

5. Page 1923 line 3 “. . . model has. . . outflow..” Where is outflow? Lines 5 and 6 on the page 1929 do not clarify the question, for my mind.

6. Page 1925 line 2-3. It is not clear why "... bottom topography and coastline correspond to the conditions during 1940 – 1955..."? Why it was not possible to specify them for 1982?

7. Page 1930 lines5, 6. It is a little bit strange that cyclonic and anticyclonic eddies are evident on the monthly mean map of currents. It means that they are quasi-permanent. What is the mechanism of their formation?

8. Page 1935lines 15, 16. Can you comment that your simulation of air sea fluxes is better than estimated with balk formulae?

9. Page 1937. The title of paragraph 4.3.1. is Mean sea level, though in the following discussion the sea level variability is considered in four points. What is the meaning of the word "mean"?

10. Page 1938. I do not understand last sentence of paragraph 4.3.2. Fig. 19 shows that amplitude of annual sea level variability is only about 25 cm.

11. Page 1945. Something is wrong on the line 15

Interactive comment on Ocean Sci. Discuss., 6, 1913, 2009.