

## ***Interactive comment on “Modeling ocean response to an extreme Bora event in Northern Adriatic using one-way and two-way atmosphere-ocean coupling” by M. Ličer et al.***

### **Anonymous Referee #1**

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The manuscript is documenting the effects of 2-way versus 1-way coupling on the ocean dynamics during an extreme cold air outbreak in the northern Adriatic. The 2012 Adriatic event is widely examined in 5-6 publications, but none of them tried to use 2-way coupling. While the manuscript is interesting and contain some fresh results, I see a number of problems that should be solved before the acceptance of the manuscript for publication. For example, there are problems in methodology (listed below) and a comparison of atmospheric models with 2-way and 1-way coupling is missing in the manuscript. I put them in order as appearing in the text:

1. Introduction and the rest. All toponyms mentioned in the text (Pomo pits – btw. Pomo pits should be renamed as Jabuka Pits or middle Adriatic depressions, as a

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geographical term comes from the country and language where the object is located; Isonzo/Soča river; Tagliamento; Livenza; Piave; and other) should be marked in Fig. 1.

2. Page 1391, lines 17-18. Bora is not a katabatic wind, see Grisogono and Belušić (2009): A review of recent advances in understanding the meso and microscale properties of the severe Bora wind, *Tellus*, 61A, 1-16.

3. Page 1392, line 6. “including our own” does not belong to introduction but to results.

4. Introduction. As I can see from figures, the topic of this paper is the DWF in the whole northern Adriatic and not in Gulf of Trieste only or off of it, as one can think reading the sentence on page 1391, lines 4-5, and other parts of introduction. So, it would be correct to present there the knowledge about the DWF occurring in the whole shelf area of northern Adriatic, that includes the freshly documented DWF site in the coastal eastern Adriatic (Janeković et al., 2014), where the heat losses were found much larger (up to 2000 W/m<sup>2</sup>) than in Gulf of Trieste during the 2012 bora outbreak, contributing to the overall DWF to about 40%, while the DWF occurring in the Gulf of Trieste does not surpass 10% of the total DWF (Janeković et al., 2014).

5. Page 1396, lines 26-28. Is there any particular reason why Neretva River is the only Croatian river introduced to the POM – also by using old climatology – while there is new climatology of Croatian rivers (also mentioned by authors) available from Janeković et al. (2014)? What about other large Italian rivers not included in the model? As far as I can see, Adige River next to Po River with average yearly discharge of 240 m<sup>3</sup>/s is not listed; also, there is not a single Italian river southeast from Reno River, while at least ten of them are listed by Raicich (1994)? Does the model reproduce well a basin-wide salinity, as no correction was applied as for temperature (page 1397)? Why did the authors use a complex hydrological model for Soča/Isonzo River and not simple river flux measurements, while the most of the salinity offset in the open northern Adriatic is probably a result of overall freshwater forcing by all Adriatic rivers (so that Soča River is not significantly affecting the area outside the gulf)? Finally, a

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spin-off time of 14 days (POM is initialised on 11 January 2012) before the bora outbreak seems to me not enough to properly set up the ocean for the bora outbreak in late January/early February 2012 – this may be tested by extending the spin-off time to let's say about 3 months, e.g. to start the model on 1 October 2011, and to compare to results obtained by the 14-day spin-off.

6. Fig. 4 and introductory paragraph to Section 4. The difference between Aladin1w and Aladin2w is an order of magnitude lower than the difference between Aladin and Vida measurements. That also applies to other results (e.g. right panel in Fig. 5). The 2w coupling resulted in no changes of the wind - why? It seems that the feedback mechanism in 2w does not change the Aladin model, while the ocean model results are quite differing between 1w and 2w. For that reason, I would like to see a throughout comparison of meteorological fields (e.g. winds or surface air temperature – e.g. spatial rmse during bora) between Aladin1w and Aladin2w, as a separate subsection 4.1, to see how the 2w coupling influences the atmosphere. And also add a comparison with previous works (e.g. Pullen et al., 2007).

7. Section 4.1. Two paragraphs here start with “Our results are consistent/similar with ...”. This is already a discussion and not the result. You should first present your results and then compare them with previous work.

8. Page 1402, line 23. I don't see in Fig. 5 how these simulations are similar to Pullen et al. (2007). Please rephrase.

9. Fig. 6. Again, I want to see the plot with differences between two models – it will make easier following the text.

10. Fig. 7. Is there any reason why heat fluxes are not computed from measurements at the buoy and plotted in the same figure, to get the evaluation of models on fluxes computed from measurements? Also, please add total heat flux to the figure.

11. Fig. 8. Please use the same scale for QH for 1w and 2w plots. Also add total heat

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flux.

12. Page 1404, line 18. “As we can see” – please change to “As it can be seen” or similar.

13. Page 1405, line 18. “Generally speaking” - delete “speaking”.

14. Fig. 9 and page 1406. Can you compute bias and rmse values for temperature at Vida buoy presented in Fig. 9 and place it into the text?

15. Referencing is not properly written in the text – an example is page 1407, line 8, which should be Malačić and Petelin (2009).

16. Fig. 11. A triangle feature at the upper left panel is strange. How many grid points you have across the C1 transect?

17. Figs. 10 and 11. Please add salinity in the figures, as the difference in evaporation and increase in salinity between 1w and 2w models are notably commented in the text (e.g., page 1411). From Table 1, the salinity difference between the models at the end of the bora episode, on 16 February, is about 0.3, implying that the cumulative evaporation 2w-1w difference during the bora episode was about 20 mm ( $0.3/38.5 \times 25$  m) – that’s a large difference! You should add some numbers like these in the manuscript to support your statements, which are currently lacking, particularly in Section 4.3.

18. There is no need to have Fig. 12, as the vertical profiles are (almost always) homogeneous, so that the Table 1 is sufficient for verification of 1w and 2w models.

19. Overall, the writing style should be improved at some places; some examples are listed above but many more are present in the text.

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