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Comment

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 #2

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General comments

It is an interesting paper, generally well written and well structured, however, several methodological problems exist. As a consequence, I cannot recommend its publications. More than a major revision is required.

Main methodological problems

A) The aim of the paper is not totally clear to me. Apparently, the authors aim at modelling an extreme event by means of a one-way and a two-way coupled atmosphere and ocean models and compare their outputs. However, the two configurations do not differ just by switching on or off the ocean-to-atmosphere channel, but also by using

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different ways of estimating the heat fluxes at the air-sea interface (see page 1400, lines 16-24; page 1406 ff), and different SSTs, namely from ECMWF/OSTIA (page 1396, line 7) for the one-way coupling and from the MFS system (1399, lines 23-24) for the two-way coupling. This makes an objective comparison impossible. The possible solution is to use the algorithms adopted in ALADIN+POM2w also in ALADIN+POM1w, instead of the formulas reported in the appendix, and the same SST data source in both configurations.

B) From another viewpoint, the two configurations can represent two different models used to reproduce an observed event, but then an extensive comparison with data is required, at least to tell which configuration gets closer to reality. Unfortunately, the comparison between model results and observed data is limited to Vida station. At page 1393, line 5, the authors say that there were several campaigns; moreover, Raicich et al. (2013) use data from a station (PALOMA) in the middle of the Gulf of Trieste, and Mihanović et al. (2013) quote Acqua Alta station, off Venice, both of which seem suitable for data comparisons. I guess that there are other available in-situ and remote observations in the domain covered by the ocean model. Moreover, Vida station seems to be very close to the coast and I wonder if the ocean properties are simulated properly, since the POM horizontal grid mesh (not resolution) is $(1/30)^\circ$, i.e. 2 nm (in latitude), while Vida is only about 2 km (approximately 1.1 nm) off Piran. Consider also that ALADIN works on a 4.4-km mesh.

Other methodological problems

a) Is this the first time in which ALADIN and POM are coupled together? The authors only quote references to Pullen (2003, 2006), Dorman (2006) and Benetazzo (2014), who use different models. If this is the first time, I wonder if it is a good idea to start from an extreme event. This comment is also related to the objective of the paper, namely a study of the models or a simulation of the event (see A, B, above).

b) The model spin-up time seems quite short (a couple of weeks).

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c) The fresh water inflow is prescribed in a complex and heterogeneous way, namely a sophisticated hydrological model for Isonzo/Soča river, observations for Po and (old) climatological monthly data for the other Adriatic rivers. I can see possible inconsistencies between real or realistic 2012 data and climatologies. Do the authors have an idea of how this complex choice can affect the results? At page 1395, lines 2-3, there is an unclear statement: 'The model calibration was performed manually'. What does it mean?

d) Page 1397, lines 19 ff: The authors do not use the MFS SST for the initial conditions because the MFS product is too cold, but MFS temperatures as used further on as lateral boundary conditions. Another consistency problem.

e) The authors themselves say that further work is preliminary or that further studies will be dealt with in future papers (page 1406, line 27; page 1413, line 28), making the paper quite preliminary.

Specific comments:

1) I am not a meteorologist, therefore I will not comment on the ALADIN performance in the area of interest, assuming that everything is fine enough, but the authors should quote some references on previous applications of ALADIN to the north Adriatic region.

2) Page 1392, line 6: Please drop 'including . . . below)'.

3) Page 1392, lines 17-18 and 21: I think that the authors simply mean 'synoptic' time scales; 'high-frequency' is vague.

4) Page 1397, lines 17-18: Please show Vida position here.

5) Page 1400, lines 1-2: This sentence can be moved to the beginning of page 1399 or even dropped.

6) Page 1400, line 19; page 1428, caption, line 4: Since both an ocean and an atmospheric model are involved, 'bottom', which is related to ALADIN, can be confused with

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'ocean bottom'. Could it be replaced by 'lowest level'?

7) Page 1400, lines 23-24: Unclear sentence.

8) Page 1402, lines 4-5: Please use '5' instead of '05', and similarly elsewhere.

9) Page 1402 ff (results): The authors should compare the performances of the two model configurations and the observations in a synthetic way, e.g. using RMSE (in time and space), STD or other indicators.

10) Page 1403, line 3: 'western Italian coast' is confusing, because west Italy is the Tyrrhenian coast; 'western Adriatic' or 'Italian' seems more clear. Similarly at line 13, 'northwestern Adriatic'.

11) Page 1403, lines 26-27; page 1426, fig. 7: What about heat flux estimates from observations? After all, the bulk formulas in the appendix were devised to work with observed data.

12) Page 1406 lines 7-10: Brackets can be dropped.

13) Page 1406 line 16: Too warm relative to what?

14) Page 1408 line 3: '...higher than ...'

15) Page 1411 line 3: Was this error not corrected by modifying the initial conditions (page 1397)?

16) Page 1411 line 18: Please drop 'whole'.

17) Page 1414, Appendix: The heat flux equations (2), (3), (4) are only similar to those found in the quoted references (Rosati and Miyakoda, 1988; Raicich et al., 2013). The formula for QB seems quite different; SST is used instead of near-sea surface temperature; $|V|$ is used instead of 10-m scalar wind speed; air temperature at sea level is used instead of 10-m air temperature. Please be more precise.

18) Page 1425, fig. 6: Velocity units are missing.

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19) Page 1427, fig. 8: Please keep the same colour scale for QB too.

20) Page 1428, fig. 9: The two POM simulations clearly show a 'tidal cycle' superimposed onto the longer-period temperatures fluctuations, as if depth was non-constant. Can the authors provide an explanation?

21) Pages 1429-1430, figs. 10-11: Although some details may be less clear, the authors should keep the same colour scales in both figures. Moreover, there might be some problems in the figure production or too few model points, because there are sharp unrealistic features.

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