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Interactive comment on "Ocean Forecasting: From Regional to Coastal Scales" *by* Emil V. Stanev et al.

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Answers on "Ocean Forecasting: From Regional to Coastal Scales" by Emil V. Stanev et al.

Anonymous Referee #2

We are grateful to reviewer for the appreciation of some parts of our work and his constructive comments, which we answer point-by-point. Some comments of technical character, which are not included in our answer, are addressed in the revised manuscript as the reviewer suggested.

My main objection to the present ms is that it lacks structure and does not present anything really new. Cursory examples are presented, with little to no supporting longterm

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statistics to back up the various conclusions. As a review paper it is too focused on the shallow water dynamics in one particular region and its very general title is not justified.

Authors:

1. In the revised manuscript we improve the structure of the paper with developing the logical links between its sections, as this was proposed also by the first referee.

2. We admit that an impression (missing novelty) could have occurred because we did not enough stress on what is the new development. In the revised paper we made clear what the novelties are.

3. In the revised manuscript we provide new statistics in form of tables and graphics to support our conclusions.

4. We made clear in the revised manuscript that the paper is about short-term predictions, not long-term ones.

5. This paper has been submitted to the COSYNA special issue of Ocean Science. Therefore we focus on the areas where most of activities of COSYNA take place that is in "one particular region". We want also to mention that it is not possible in one paper to address in sufficient detail many different coastal areas. Our choice was one area, but several different aspects.

6. Following the comments of referees we changed the title.

I recommend . . . that the authors instead resubmit a more focused study on the shallow water dynamics in this region, with more emphasis on verification and less emphasis on specific examples.

Authors:

1. The first submission was exclusively on the shallow water dynamics.

2. We provide in the revised manuscript more verification material, numbers for statis-

tics, tables, etc. and respective explanations.

3. The number of examples considered has been reduced.

4. The presentation is confusing and the text is not properly structured. Again, restricting focus to one specific dynamical problem would help increase the clarity of the presentation.

Incosistent use of abbreviations adds to the confusion (e.g. "SAR" vs "search and rescue").

Authors:

1. In the revised manuscript we develop the logical links between individual parts of the paper.

2. We formulate for each sub-section more clearly what is the specific dynamical problem addressed.

3. The presentation has been restructured (sub-sections omitted, other sub-sections displaced, some sub-sections are restructured, in some others more weight has been given to issues suggested by referees).

4. We avoid using misleading abbreviations.

- Central information about the various modeling systems is only given in the appendix. The level of detail is unsatisfactory and the ms cannot stand on its own in its present form. A proper model comparison will require a more elaborate discussion about their differences, for instance the impact of using hourly vs six hourly atmospheric forcing.

Authors:

1.As suggested also by the referee #1, we present in the revised manuscript a table of models used in the paper and their most important details.

2. We increase (wherever necessary) the level of details given for the individual models

in order to balance the deepness of presentation of all models almost the same.

3. As we stated in the first submission, model inter-comparison is not the aim of this study. Explanation about this strategy has been presented in the first submission (see section 2.2.2 where the rationale is presented). We want to remind that outputs of some operational models have been used and it is not possible to change the way how atmospheric forcing has been used.

4. Our strategy was to use forcing data with as fine as possible resolution in time.

It is also difficult to keep track of which model is used for what purpose as the authors jump back and forth between them in the examples.

Authors: We checked carefully all sections and wherever this has not been explained clearly, we provide the necessary information.

- Errors of representativeness, which becomes an important issue when downscaling data assimilative models merits a discussion, but is not mentioned here.

Authors: In the revised manuscript we provide new skill estimates (in particular as far as data assimilation has been concerned) and refer to previous studies on this.

- References are missing in several places, e.g. pages/lines 2/15, 3/24, 7/12; there are several errors in the citations (e.g. 5/24, 10/28); and reference to unpublished material makes no sense (9/4).

Authors: We are thankful for this comment and we did all proposed corrections in the revised manuscript.

- The HF radar assimilation technique based on the method of Stanev et al (2015) may be well justified for use in this region, but might be less useful in regions where baroclinicity and/or the influence of complex topography dominates.

Authors: We focus on the German Bight in this paper. Addressing other regions would increase the diversity of addressed issues, which we, following referees' comments,

want to avoid. Following this comment we mention in the text that the demonstration of skill is valid for the specific area. Further applications of proposed method to different regions (e. g., regions dominated by pronounced baroclinicity) need additional analysis. As said above in the limited space of this paper we cannot address many different areas, also because availability of HF radar data is also a problem.

It would be good to see an assessment of the impact of HF radar DA on storm surge predictions instead of the (very short) discussion about search-and-rescue support.

Authors: In the revised paper we address the capability of HF radar data to detect changes in surface currents during storm surge periods. In this way we also increase the inter-connectivity between different sections.

Several published papers deal with the impact of HF radar data assimilation on current predictions, e.g. Barth et al (2008, JGR, using ADCP for verification), Yaremchuck et al (2016, DSR II, using drifters), Sperrevik et al (2015, OS, using both drifters and ADCP), so that the cursory example presented here does not really provide anything new.

Authors:

1. We stressed in the first submission that the novelty we address is in the forecasting at intra-tidal time scales. Most of the past studies use de-tided HFR data. In the revised manuscript we emphasize on this novelty (as this was one of the suggestions in the general comments).

2. In section 3.2 we include a short presentation of earlier works in this field and cite the proposed ones.

- The apparently small impact of in-situ data (ferrybox) vs the OSTIA product indicates that the DA system is not working optimally. I would expect in-situ data to be rather more valuable, but again, very little in the way of statistics is presented, e.g. innovations vs analysis increments and their temporal and spatial distributions. Mention could also be made about rapid update cycles, which is used successfully by e.g. the KNMI in their

regional NWP system to maximise the use of observations in small model domains (deHaan, 2013, QJRMS).

Authors:

1. We explain in the revised manuscript the reasons of “small impact of in-situ data” providing more statistics.

2. We show a comparison between free run, OSTIA and fine-resolution temperature data.

3. Additionally we refer to earlier publications (Grayek et al., 2011; Stanev et al., 2011) where more details are given about the systems’ performance and skill estimates.

4. We refer in the revised manuscript to the publication mentioned by the referee.

- The "two-way nesting" method described in Sec. 2.3 differs from the full online nesting implemented in e.g. ROMS and AGRIF. I assume the nudging based method presented here will in practice work as a low pass filter when information is exchanged between parent and child grids, and I would like to see what the impact is on fast time scales such as tidal wave propagation.

Authors: Answering this question, we provide in the revised manuscript more details about temporal variability (tidal analysis and variability of salinity in the transition area), which demonstrates that the proposed method does not the impact negatively dominant dynamics in the transition area.

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