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Geoscientific Model Development Discussions

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Interactive comment on "Dealing with discontinuos meteorological forcing in operational ocean modelling" *by* Bjarne Büchmann

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REPLY TO REVIEWER #2

If I read the comments of Reviewer #2 correctly, then there are the following concerns:

1. The limited analysis, which the reviewer finds is "not enough to demonstrate the added value"
2. No results are given for "ramp0"
3. There is doubt about how the method is actually applied.

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Discussion paper

I would very much like to respond to / explain each issue and subsequently suggest changes to the manuscript. I hope that the reviewer can then comment on the suggestions before I implement the changes to the manuscript.

Add 1:

I have tried to keep the manuscript brief – in the spirit of creating a technical note. It was considered to include more data, but the decision was taken that more data might not necessarily make the manuscript more usable. Adding data for inner domains, such data would (still/also) be selective: the actual choice of meteo and ocean models would be specific to our present setup, and the gauge positions and periods would to some extend be arbitrary. Data from the "inner domains" (NS1C, DK600, see fig 5) might show a stronger response. However, that would introduce one or two extra ocean models as well as at least one extra meteo model, as another forcing (DMI-HARMONIE-NEA) is used for the inner domains.

We (FCOO) routinely compute annual statistics for our models compared to measured elevations. For Wick, the RMSE error on surge (not tide) is typically in the order of 6-7 cm. The spurious waves stemming from the meteo discontinuity are smaller (several centimeters, see Fig 11B), so while they may not be the most important contribution to the model error, they should not be ignored either.

The present choice of data was taken for the following reasons:

1. ECMWF-IFS is a well-known and widely used model – presumably more so than data from a specific national weather centre.
2. The actual size of the discontinuity varies with the meteo model/setup (as well as with time and space) and – at least to some extent – on the ocean model specifics, i.e. the magnitude of the problem will differ from case to case. There-

fore, the choice was made to focus on the principles rather than specific values of the data.

As it happens, the ECMWF-IFS model is rather well-behaved, presumably because the results are computed rather late in the cycle, such that the first hours show only small change from previous forecast. I have worked with other meteo-datasets, where the problem was much more pronounced.

3. It is the goal of the paper to show that "there is a problem", and suggest a simple method to deal with it. The actual choice of ramping length (N) should depend on the application (weather model, update frequency, ocean model etc). In my opinion, "noramp" should never be used, as there is zero computational overhead going from noramp to ramp0. And modellers should at least be aware that there could be a problem – and in many cases N>0 should be considered.

4. It has not been my intention to show that ramping alone provides “better” results in term of, say, lowered RMS errors compared to elevation gauge measurements, or show improvements on particular time series.

Add 2:

The reviewer writes that "no results are shown for the method the most commonly used in the operational oceanographic community according to the author himself, i.e., the ramp0 method".

Firstly, it is not my intent to claim that ramp0 is "the most commonly used [method]". It is my impression (personal communications), that the ramp0 method is much used in *hindcast*, as written on p3, l19-20: "This approach [ramp0] corresponds exactly to running a hindcast based on the first six (hourly) fields of each meteo forecast, which seems to be a very common choice among ocean modellers."
However, in operational/forecast mode, even ramp0 would require that the hotstart

files are not written at epoch t=0, but at an earlier time (say, t=-1H). And unfortunately, it seems that that t=0 is commonly used for the hotstart files, thus leading to "noramp". Thus, it is my best guess (claim) that "noramp" is rather common in operational /forecast mode. For newer operational/forecast setups, where data assimilation of the recent past is included, the hotstart time is presumably "well before epoch". In such cases, even operational /forecast will typically end with a "ramp0" scenario.

Add 3:

The reviewer writes that "Figure 4 is supposed presenting the method for ramp3 and ramp6. If ramp3 seems really to be a linear interpolation between the field at t-1H and that at t+4H this seems not to be the case for ramp6 (linear interpolation between t-1H and t+6H). How is the method really working?"

This seems to be a point of genuine misunderstanding of the method. Obviously, this falls back on the author/manuscript, which means that I need to improve the section describing the method itself (see below). Basically, there is no interpolation in time taking place. Rather, each meteo-field is recomputed as a "linear combination" (weighted mean) between old forecast and new forecast – each taken for the same time value (hour of the clock). For instance, ramp3 uses 1/4 new value and 3/4 old value (ie 25/75) at t=0H, 50/50 at t=+1H and 75/25 at t=+2H.

The illustration for ramp3 (Fig 4) appears linear only because the two (old and new) forecasts exhibit near-linear behaviour over these few time slices.

SUGGESTED REVISIONS

Based on the reviewer comments, I plan to make the following changes to the manuscript. If possible, I should like (short) reviewer input, in order to limit the total review-work.

1. As the "ramp3" part of Fig 4 seems to be misleading in the present version, I suggest striking that particular subfigure, such that Fig 4 will depict only ramp6.
2. I will improve the explanation of the method using a simple equation, adding around page 4 line 10:

If M_n^i denotes the n'th field of forecast no. i, so that M_{n+6}^{i-1} and M_n^i are consecutive forecasts for meteorological fields at the same time (hour on the clock), then the rampN process computes updated fields as

$$\widetilde{M}_n^i = \frac{N-n}{N+1} M_{n+6}^{i-1} + \frac{n+1}{N+1} M_n^i \quad , \quad \text{for } n = 0, \ldots, N-1 \tag{1}$$

For $N > 6$, it is necessary to make the computation recursive, so that Eq. 1 is used for the first ramping ($i = 1$), while later epochs use $\widetilde{M}_{n+6}^{i-1}$ in place of M_{n+6}^{i-1}. It should be noted that the suggested ramping process does not imply interpolation in time.

3. As suggested by the reviewer I plan to include "ramp0" data. I suggest to simply include a third panel on Fig 11, depicting difference between ramp0 and ramp6 data. That should indicate also to what extend ramp0 "is enough" compared to ramp6. Also, I will remedy the sentence on p3, l19-20 to stress that the "common choice" also is for hindcast:

This approach [ramp0] corresponds exactly to running a hindcast based on the first six (hourly) fields of each meteo forecast, which – for hindcasts – seems to be a very common choice among ocean modellers.

4. I will include data (ocean model computed elevation) for one or two stations in shallower water/inner domain, presumably the Kattegat and/or the south-western Baltic Sea, see suggestions from reviewer #1. These data will include effects of ramping (or not) of both the outer (ECMWF-IFS) and inner (DMI-HARMONIE-NEA) meteo models. Thus, the results will be less general, but maybe give a better sense for the scale of the problem. For DMI-HARMONIE-NEA we routinely use ramp9, and I will compare to noramp and ramp0 data.
   For all stations I will include the typical RMSE error from our annual statistics, such that a comparison can be made between the size of the spurious waves – and the typical model error on the station.
   I do not plan to include observation data in the figures (time series).

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