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Geoscientific Model Development Discussions

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

Bjarne Büchmann

bjb@fcoo.dk

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REPLY TO REVIEWER #1

If I read the comments of Reviewer #1 correctly, then the major general comment is to request additional results to gauge "whether the proposed ramping solution actually leads to any improvements in the ocean model forecasts". In particular the reviewer:

1. would like to see results for inner domains – with a query on why the experiment has not been made for these domains.

2. would like comparisons with measurements to see "if the ramping has led to an improvement".

I would very much like to respond to / explain each issue, and only 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:

We (FCOO) have been using meteo ramping for all our model setups (NA3, NS1C and DK600) for well over a decade. In that period, we have been through several "generations" of DMI-HIRLAM models (E15, T15, S05, S03), before recently (2018) switching to ECMWF-IFS for the outer domain and DMI-HARMONIE-NEA for the inner domains.

As it happens, the ECMWF-IFS model is rather well-behaved, presumably because the results are computed rather late in the 6-hour 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 (in particular the now-retired DMI-HIRLAM-T15). Even so, ECMWF-IFS is a well-known and widely used model – presumably more so than data from a specific national weather centre, which is why I opted to disseminate this particular case.

For 3D (baroclinic) model setups (eg. NS1C, DK600), there is an additional complication, as even the slightest change of forcing or initial conditions, is likely to trigger a different outcome of the stochastic process, which is the model, see e.g. Büchmann and Söderkvist (2016) [DOI: 10.3402/tellusa.v68.30417]. In essence, the stochastic eddies might be positioned differently in two simulations, even if the changes to input are only at machine precision, and this may cause (small) changes also to the computed elevations. Fortunately, the effect on the elevations in the Kattegat area is rather small – sub-millimeter scale – so in principle it should be feasible to include modelled elevation for one or two positions from the inner domains: Kattegat and/or the south-western Baltic Sea.

Add 2:

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. That is why I have not included measurements – I find them irrelevant for the present discussion, and they could simply muddy the discussion (but see suggestions for changes later).

We 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.

PLANNED REVISIONS

A:

I will make sure to note that FCOO operationally use ramping on all received meteo data before the data are used for modelling. I will also add a comment that the ramping length depends on our experience with the data set.

I will add a remark to the point that if the old and new meteo forecasts are very similar, ie. if the discontinuity is small, then the effect of ramping is also small (making a weighted mean of two similar results). In essence, the suggested ramping is not harmful, but "kicks in" in the cases, where the discontinuity is significant.

B:

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. 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 as well as to ramp0 (see reply to reviewer #2) data.

I plan to include the data in the same way as Fig 11, ie. time series of elevations and differences. I do, however, not plan to disseminate statistics on the alternative meteo model (DMI-HARMONIE-NEA).

C:

To give a better feel for whether this is "a problem in real forecasting", I will – for each station shown – include typical model errors (RMSE, compared to observed elevation), which can then be compared to the scale of the spurious waves.

COMMENTS ON TECHNICAL CORRECTIONS

1. The spelling corrections are duly noted. Thanks so much.

2. A typo in the title is just plainly embarrassing – sorry about that.

3. I have considered the possibility to add the “neatl-box” on figs 6, 7 and 9. Presently, I tend to think that the additional information “ready at hand” is not worth the increased complexity of the figures. In order to increase the readability, all figures already use exactly the same projection, although Fig 5 is slightly larger, as there is no colour legend. Unless the reviewer really would like to see this, I will not include the “neatl box” on additional plots.

4. I will try to find better colour scheme for the lines of Fig 10, replacing green with, say, orange.

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