

Revised manuscript “Wave-atmospheric modelling, satellite and in situ observations in the Southern North Sea: the impact of horizontal resolution and two-way coupling” by Kathrin Wahle et al. presents interesting results of how two-way atmosphere-wave coupling improves the coastal wave forecast. This is an important step in development of coupled models for short-term coastal forecasting with high-resolution, especially as Authors have been able to show that the very high-resolution coastal applications also benefit from the two-way coupling. The revision has improved the manuscript, making it more focused and easier to read. The Authors have also well taken into account the reviewers comments and suggestions. There are, however, still few places where further clarification is needed (please see my detailed comments below). Also, I suggest that some copy editing is done to the text before publication. Some sentences are quite long and therefore not easy to follow and there is also some repetition.

Some detailed comments:

Section 1. Introduction: This section has improved a lot, and is now better structured and easier to follow. It is, however, quite long and has repetition. I think it would benefit of some copy-editing.

Section 1, lines 58-59: alternative to fully-coupled ocean-atmosphere model? Would this model include also waves? And why should we have alternatives, shouldn't we aim for the fully coupled models.

Section 2.2, line 169: Should it be WAM4.5.4?

Section 3.1, line 260: the situ-data → the in-situ data?

Section 3.2, first paragraph: The bias is calculated as measured minus modelled value and following this, in text it is said that altimeter data underestimates the modelled values. Shouldn't this be said the other way around? E.g. modelled values are overestimated compared to the satellite measurements. In most of the cases I'd assume the altimeter data to be more accurate than the modelled data and it is said to be the dataset against which the model is verified.

Section 3.2, first paragraph: If the Authors do not trust the Cryosat-swh, why is it used for validation in the first place. Wouldn't two altimeter datasets be enough for validation? Anyhow, the explanation related to this in lines 287-297 is bit complicated to follow. Should the reader disregard the results from this comparison or interpret them with care?

Section 3.2, lines 285-287: Quite a long sentence, could be split to two parts

Section 3.3, second paragraph: The reason behind the better behaviour of the high-resolution model is probably mostly due to the better description of the bathymetry in the area, not the high-resolution *per se*. It is implicitly mentioned by the Authors, but it could be stated more clear.

Section 4, line 414: Why not simply say, that the fetch is too short for the waves to evolve.

Section 5, lines 446-449: Quite a long sentence, could be split to two parts.

Section 5, lines 473-474: What is meant by potential uncertainties of shallow water in the wave model? Is this related to the description of bathymetry or to the wave model source terms related to shallow water physics?

Tables 1-3: Table captions should explain what the red and green colouring means.

Figure 3: Please explain the marked overflight also in the Figure caption.

In the Figure and table captions there is a mixture of terms “wave height” and “significant wave height”. Preferably “significant wave height” should be used in all of them.